

THE PSYCHOLOGICAL BULLETIN

THE SCIENTIFIC INTERESTS AND SCIENTIFIC
PUBLICATIONS OF THE MEMBERS OF
THE AMERICAN PSYCHOLOGICAL
ASSOCIATION, INC.

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When the American Psychological Association adopted its first constitution in 1894, Article I read as follows: "Object.—The object of the Association is the advancement of Psychology as a science. Those are eligible for membership who are engaged in this work." The Certificate of Incorporation of the Association adopted in 1925, and under which the present Association is operating reaffirms this principle and states in Article III "That the object of this society shall be to advance psychology as a science." In the present By-Laws, Section 2 of Article I reads in part: "Members of the Association shall be persons who are primarily engaged in the advancement of Psychology as a science." And Section 3 of Article I also reads in part: "Associates shall be such other persons as are interested in the advancement of Psychology as a science and who desire affiliation with the Association for this reason."

It would seem, then, that the "advancement of Psychology as a science" has been the expressed primary motive of the Association throughout its more than 45 years of existence. It is obvious that such scientific advancement may be accomplished in a great many ways,—by teaching, by practical applications (clinical, educational, industrial and otherwise) and certainly by research. The writer has no criticism of the many applications which have been made in many fields of human endeavor. How great a contribution these applications have made to human efficiency and to human happiness cannot

well be overestimated. The training of future psychologists and of individuals in psychological techniques is a great contribution to human welfare. But such matters are more or less ephemeral for evaluation and their contribution to the "advancement of Psychology as a science" cannot be easily or accurately measured.

A study of the research activities of the members of the Association would seem the best method of evaluating how well they live up to the expressed purpose of the organization. It must be granted that much research never comes to publication and it must be also granted that many publications probably do not extend the scientific horizon of psychology. But the consideration of such matters would involve judgments on the part of the writer. A study of published titles can be the only objective method of obtaining this information. In this study, a title must be considered a title, whether it be a casual half page note in some journal or the report of a long experiment descriptive of methods and results which may lead to the development of new and important research techniques or the opening of a new field of psychological research. The evaluation of each title would involve subjective estimates on the part of the writer which he wishes to avoid.

In the present article, we propose to do two things: (1) to study the expressed research activities of the Members and Associates of the American Psychological Association as these may be culled from the *Yearbooks* of the Association and (2) to study the actual publications of the Members only.

Research interests are asked for each year from every Member and Associate for publication in the *Yearbook*. These notations of research interests first appeared in the 1918 *Yearbook*. In the present study, we have considered these self-expressions of research interest for the years 1918, 1927, and 1937—3 periods separated by practically ten-year intervals. The results for Associates are included only in the 1937 summary because the numbers of Associates in 1927 were too small to be considered an appropriate sample and in 1918 this class of membership did not exist.

In Table I will be found the data for Members for each of the 3 periods and for Associates during the last year. The table contains the number of individuals, the total number of expressed research interests, the average number of research interests, and the total number of research fields mentioned.

In 1918, the 367 Members reported only a total of 521 research interests, or an average of only 1.4 interests for each member. Only

54 different titles of research interests were listed by this group. By 1927, 516 Members reported 1,179 research interests or an average of approximately 2.3 interests for each member. In this year a total of only 62 fields of research was listed. This number is very little larger than that of 10 years before. In 1937, the 587 Members reported 1,523 research interests or a per capita average of 2.6. For this group, a total of 123 titles of fields of research interest was noted. In 1937, the 1,551 Associates reported 3,695 research interests or an average of 2.4 for each Associate. This group reported a total of 163 fields of research interest.

TABLE I
REPORTS OF RESEARCH INTEREST

Date	Number of Individuals Reporting	Total Number of Fields Reported	Average Fields Per Capita	Total Number of Different Fields Reported
1918.....	367	521	1.4	54
1927.....	516	1,179	2.3	62
1937 (Members)...	587	1,523	2.6	123
1937 (Associates)...	1,551	3,695	2.4	163

It is worth noting that the number of Members has increased steadily during this twenty-year period but the greatest increase in the size of the membership was during the first of these 2 decades. There has been also a steady and more rapid increase in the number of research interests reported on the average by the Members during this period as indicated by both the actual number of interests reported and by the average per member. Although there was little increase in the number of different fields of research interest reported by the Members from 1918 to 1927, in the next decade the numbers of fields were almost double that reported 10 years before. And the Associates in 1937 contribute almost 25% more fields of research than the much smaller number of Members in the same year. These results would seem to indicate that in the last 10 years both Members and Associates have wished to report very specific research fields and interests such as "disabilities in reading," "electro-physiology," "factor analysis," "instructional films," "student problems," and "nerve end organs." It would seem, therefore, that during the last 10 years, psychologists have come to consider that they are working on very specific problems within very circumscribed limits, while formerly they were content to describe their research activities as within a much larger "field" of interest and endeavor.

In Table II are listed the frequencies with which Members and

Associates in 1937 listed research interests. The numbers vary from 0 research interests to a single Associate listing 12 different research interests. The mode for both classes of membership is at 2 fields. The averages have already been indicated in Table I as 2.6 for Members and 2.4 for Associates. Some 40 Members and 110 Associates report no research interests or activities whatsoever. At the other end, 3 Members and 6 Associates report research interests in 7 fields; 1 Member and 4 Associates in 8 fields; 2 Members and 1 Associate in 9 fields, and 1 Associate records research interests in 12 different fields.

TABLE II
FREQUENCY OF RESEARCH FIELDS INDICATED BY MEMBERS AND ASSOCIATES
IN 1937

Number of Fields	Members		Associates	
	Number	Per Cent	Number	Per Cent
0	40	7	110	7
1	83	14	298	19
2	165	28	474	31
3	163	28	391	25
4	84	14	175	11
5	38	7	70	5
6	8	2	21	1
7	3	1	6	0
8	1	0	4	0
9	2	0	1	0
10	0	0	0	0
11	0	0	0	0
12	0	0	1	0

In Table III will be found the frequency of particular interests for Members for the years 1918, 1927, and 1937 and for Associates in 1937. The rank order, number reporting, and per cent reporting each particular research interest is indicated. One can thus compare the research popularity of the different fields for Members over the twenty-year period, and one can also compare the research popularity of Members and Associates in 1937.

A study of this table indicates that the Experimental field has always been in first place for Members during the twenty-year period. The number of Members reporting experimental research interests has increased steadily. On the other hand, the percentage so reporting was higher in 1918 than in either of the 2 subsequent periods studied. However, if one adds Physiological to Experimental in 1937 for Members, the total percentage is raised to 44, which is 7% higher than this combined total in 1918.

Tests and Educational Psychology have been in either second or third place for Members during the 3 periods studied. In 1918, Tests had third rank order and Educational second, but these relations were reversed for both 1927 and 1937. For Tests, both numbers and percentage of Members reporting increased from 1918 to 1927. From 1927 to 1937 the number remains almost the same while the percentage decreases. The percentages and rank orders are identical for Tests for Members and Associates in 1937. There is a slightly different trend for Educational Psychology. The numbers and percentages both increase from 1918 to 1927; while from 1927 to 1937 there is a slight decrease in numbers and, with the general increase in the size of the membership, there is a marked decrease (5%) in the percentage of Members interested in the Educational field. The percentage of Educational Psychology as a research field is almost identical for Members and Associates in 1937.

In 1918 only 15 Members or 4% indicated an interest in Clinical Psychology. This was increased to 99 or 19% in 1927, and to 107 or 18% of the Members in 1937. Thus the popularity of this field rose for Members from sixth rank order in 1918 to fourth rank order both in 1927 and 1937. It is in the Clinical field that there is the most marked difference in popularity between Members and Associates. For this latter group, Clinical Psychology has the first rank with 28% of the Associates reporting research interests in this field—or 10% more than the Members of the same year.

Social Psychology has steadily gained in popularity among the Members. The rank order has risen from eighth to sixth and finally to fifth place.

The numbers have risen from 9 to 53 to 78 and the percentages from 2 to 10 to 13 during the twenty-year period. In 1937 the Members and Associates show identical rank orders and percentages of interest in this field.

Abnormal Psychology has increased in popularity among the Members from ninth to seventh and finally to sixth rank order. The number of Members interested has increased from 8 to 50 to 55. The percentages rose from 2 to 10 between 1918 and 1927 but dropped to 9% in 1937. For Members and Associates in 1937, the rank orders are identical, but 3% more Associates report an interest in Abnormal than the Members of the same year.

Physiological research has had an enormous increase in interest for Members over the 20 years studied. In 1918 it ranked twenty-third with only 1 person reporting. Certainly more than 1 Member

TABLE III
RESEARCH INTERESTS OF MEMBERS AND ASSOCIATES

Research Field	Members 1918 N=367			Members 1927 N=516			Members 1937 N=587			Associates 1937 N=1551		
	Rank Order	Number	Per Cent	Rank Order	Number	Per Cent	Rank Order	Number	Per Cent	Rank Order	Number	Per Cent
Experimental.....	1	137	37	1	152	29	1	203	35	3	373	24
Tests.....	3	79	22	2	149	29	2	145	25	2	396	25
Educational.....	2	81	22	3	135	26	3	123	21	4	353	23
Clinical.....	6	15	4	4	99	19	4	107	18	1	428	28
Social.....	8	9	2	6	53	10	5	78	13	5	200	13
Abnormal.....	9	8	2	7	50	10	6	55	9	6	192	12
Physiological.....	23*	1	0.3	13	26	5	7	53	9	10	118	8
Theoretical.....	4	54	12	8	42	8	8	52	9	20	48	3
Genetic.....	18*	2	0.5	10	36	7	9	45	8	9	119	8
Vocational.....	24*	1	0.3	5	55	11	10	45	8	8	147	9
Animal.....	5	32	9	15	24	5	11	42	7	12	93	6
Industrial.....	21*	1	0.3	11	35	7	13	36	6	13	90	6
Comparative.....	20*	1	0.3	14	20	4	14	34	6	17	59	4
Statistics.....	0*	0	0	16	21	4	15	34	6	11	101	7
Child.....	14	3	1	24	11	2	16	33	6	14	81	5
Personality.....	0*	0	0	29	5	1	19	24	4	18	52	3

* In this table only 16 fields of purely psychological interest are included. Other fields such as philosophy and education were reported. Hence rank orders greater than 16 are included because of these non-psychological fields which were eliminated. Also a zero in the rank orders indicates that this field was not mentioned by anyone as a research field.

was doing physiological research in this year, but it was not recognized as a separate and special field. By 1928, it had risen to thirteenth place with 26 persons or 5% reporting and, by 1937, it had risen to seventh place with 53 Members or 9% reporting Physiological interests. Among Associates for this latter year, it ranks in tenth place with 8% reporting.

Theoretical has had an exactly opposite trend. In 1918 it ranked in fourth place and has dropped to eighth place in both 1927 and 1937 and to twentieth place among the Associates for the latter year. The numbers and percentages both dropped for Members between 1918 and 1927. Both increased slightly in 1937, but neither the numbers or percentage of the Members reporting a Theoretical interest are as great as those 20 years before. In 1937 there are actually fewer Associates reporting theoretical interests than Members and, when one considers the much larger number of Associates, the percentages are 9% for Members and only 3% for Associates.

The Genetic field shows an increase of popularity for Members and great similarity for Members and Associates in the last year studied.

Vocational Psychology shows an increase at first, followed by a decrease for Members during the 20 years. There is a slight increase of popularity of Associates as compared with Members in 1937.

Both the rank order and the percentages for Animal Psychology are greatest in 1918. There is a marked decline for both of these indices in 1927 with only a partial recovery by 1937. The rank orders and percentages in this field are very similar for Members and Associates in 1937.

Industrial Psychology in 1918 was reported as a research field by only 1 Member. This was increased to 20 or 4% in 1927 and to 34 or 6% in 1937. Only 4% of the Associates report this activity as a research interest in the latter year. Child Psychology was reported successively by 3 Members in 1918, by 11 in 1927 and, at the end of the last decade, this number had trebled to 33 or 6% of the Members—a percentage almost identical to that of the Associates in the latter year.

In 1918 not a single Member reported a research interest in Statistics! This was increased to 11 or 2% in 1927 and to 33 or 6% in 1937. There were 5% of the Associates reporting this research field in 1937. Also in 1918, not a single Member reported a research interest in Personality and only 5 Members or 1% reported this interest in 1927. This number has been increased to 24 Mem-

bers or 4% in 1937, while 3% of the Associates report this interest in this latter year.

These results indicate that increases in interest, as judged by the percentages of Members reporting, are to be noted for the following fields over the twenty-year period studied: not markedly for Tests and Abnormal but very markedly for Clinical, Social, Physiological, Genetic, Vocational, Industrial, Comparative, Statistics, Child and Personality. An actual loss of interest, as measured by the same criterion, is indicated for Theoretical Psychology. The Experimental, Educational and Animal fields show percentages of interest which do not vary more than 2% between 1918 and 1937. In the Experimental field, there was a decrease in 1927 which was made up by 1937; while in the Animal and Educational fields there was an increase of popularity in 1927 which was more than lost by 1937 in both cases.

When one compares the percentages for Members and Associates in 1937, the following fields do not vary beyond the limit of 2%: Tests, Educational, Social, Physiological, Genetic, Vocational, Animal, Industrial, Comparative, Statistics, Child and Personality. There is a markedly greater interest in Clinical Psychology among Associates and an increased interest, but less marked, in Abnormal Psychology. There is markedly less interest among Associates in Experimental and Theoretical Psychology than among the Members in 1937.

So much for the statistical treatment of what the Members and Associates say about their research interests. It has seemed worth while to determine what these same people have actually published in the way of psychological research. A period of 5 consecutive years seemed a large enough sample to determine the frequency of publication for any one individual. The data were obtained from the *Psychological Abstracts* for the years 1932-1936, both inclusive. In order to make these results as objective as possible, each title was recorded as a single title and no consideration was given either to the size of the contribution or to its apparent importance. A title was a title if it was a part-page note or a book of the first importance. The titles were classified in the categories of the *Abstracts'* classification. This classification was changed in the 1937 volume. Results were recorded only for Members in 1937 because the number of Associates was so great and also because the Associates are, on the whole, a less mature group, many of whom have published little or nothing at all.

The results of this analysis of titles is found in Table IV. The total of 587 individuals who were Members of the Association in the 1937 *Yearbook*, published a total of 3,963 titles listed in the *Abstracts* during the period 1932-1936, or an average of 6.75 titles per Member. This table, however, shows the wide range of varia-

TABLE IV
NUMBER OF TITLES PUBLISHED IN FIVE-YEAR PERIOD
MEMBERS IN 1937 ONLY

Number of Papers	Number of Members	Number of Papers	Number of Members
0	126	21	4
1	53	22	4
2	46	23	4
3	41	24	5
4	34	25	1
5	30	26	2
6	20	27	2
7	27	28	2
8	29	29	1
9	36	30	1
10	20	31	2
11	15	32	1
12	12	34	1
13	8	35	1
14	11	37	2
15	10	38	1
16	11	39	1
17	7	41	1
18	5	43	1
19	5	49	1
20	4	Total	3,963

bility of frequency of publication. While 126, or 22%, of the Members published nothing at all during this five-year period, one individual published 49 titles or an average of almost 10 titles each year. While the average of this distribution is 6.75 titles, the mode is zero and the median value is only 4 titles. The table thus indicates the very wide range of variability of output.

Table V indicates the consistency of publication by indicating the

TABLE V
CONSISTENCY OF PUBLICATION
MEMBERS 1937 ONLY

Number of Years in Which Published	Number of Members	Per Cent of Members
0	126	22
1	67	11
2	66	11
3	94	16
4	112	19
5	122	21

number of years in which each of the Members published. While 126 or 22% published nothing at all during this period, 122 or 21% of the Members published at least once during every one of the 5 years. And 112 or 19% published at least once during 4 of the 5 years. Thus 40% of the Members missed publication only once during this period or not at all.

Of perhaps more interest are the results given in Table VI, in which this total of publications is broken down into years and also into the categories of classification as given in the *Abstracts*. May we repeat that in order to make this study as objective as possible, no effort has been made to go beyond the *Abstracts'* classification. But it should be noted that both the classification and the editorship

TABLE VI
NUMBER OF PUBLISHED TITLES BY FIELDS
MEMBERS 1937 ONLY

Field	1932	1933	1934	1935	1936	Total 5 Years
General.....	64	107	84	102	96	453
Sensation and Perception.....	51	52	97	82	72	354
Feeling and Emotion.....	13	20	15	16	9	83
Attention, Memory and Thought.....	23	37	54	44	34	192
Nervous System.....	9	10	13	19	28	79
Motor Phenomena and Action.....	38	78	93	92	79	380
Plant and Animal.....	71	88	88	110	81	438
Evolution and Heredity.....	5	7	8	3	17	40
Special Mental Conditions.....	35	42	9	8	13	107
Nervous and Mental Disorders.....	27	32	33	41	25	158
Personality and Character.....	51	46	35	132
Social Functions of the Individual....	52	69	82	71	86	360
Industrial and Personnel Problems....	22	36	56	28	27	169
Educational.....	69	80	78	90	85	402
Biometry and Statistics.....	19	29	18	15	23	104
Mental Tests.....	32	41	33	21	19	146
Childhood and Adolescence.....	51	65	82	84	84	366
Totals by Years.....	581	793	894	872	813	3,963

of this journal have remained constant over the period (except for the inclusion of a special category of Personality and Character in 1934) and so one may assume considerable constancy of definition of the different categories underlying the classification.

The results show an increase in the total number of titles during the first 3 years—a marked increase from 1932 to 1933 and less marked for the succeeding year. After the maximal number of titles published by this group in 1933, there are slight decreases for each of the 2 succeeding years.

The largest number of titles in any category is in the General

classification, which includes a miscellaneous collection of topics such as textbooks, apparatus notes, systematic articles and notes, biographical notes and the like. The Members published a total of 453 titles in this category over the five-year period. There is no apparent consistency of trend of output for these years, however. The maximal number of General titles occurs in 1933 with the second high point in 1935.

The next largest number of titles appears under Plant and Animal Psychology (which contains few if any 'plant' titles) with a total of 438 over the five-year period. The number of titles rises to a maximum in 1935 with a slight recession in the last year. Next highest is Educational Psychology with a total of 402 titles. In the first year, the smallest number of titles was published in this field, while during the last 4 years the numbers have been greater and fairly constant.

Motor Phenomena and Action is next in rank order with a total of 380 titles. There is a very marked increase in number from 1932 to 1933 with more than double the number of titles in the latter year. The maximum is reached in 1934, practically maintained in 1935, with a slight recession in 1936. Next in rank order is the category of childhood and adolescence with a total of 366 titles. The number in this field steadily increases over the period studied with the maximum in the last 2 years. Social Functions of the Individual totaled 360 titles over the five-year period, with an irregular increase to a maximum in the last year.

The category of Sensation and Perception ranks seventh with a total of 354. The number of titles is very low during the first 2 years, while in 1934 the number is almost double that of the year before and is a maximum from which there has been a steady decline during the last 2 years.

All of the other categories of classification have markedly smaller numbers of titles—the largest of these containing only half as many as the smallest of the categories already discussed. The largest of the latter group is Attention, Memory and Thought with a total of only 192 titles. The numbers are very low in 1932, more than doubled to a maximum in 1934, with a steady decline in the last 2 years. Industrial and Personnel Problems rank next with 169 titles. This category again starts very low in 1932 and builds up to a maximum of more than double this number of titles in 1934 with a very marked decline in each of the last 2 years. Nervous and Mental Disorders comes next with 158 titles with a steady increase to a

maximum in 1935 and a sudden drop to an actual minimum in the last year. Mental Tests ranks next with 146 titles. Here the maximum is in 1933 with a very marked decline during the last 3 years.

Next in rank order is the category of Personality and Character with a total of 132 titles. This category was added only in 1934, with the maximum that year and a steady decline in the number of titles during the last 2 years. Special Mental Conditions has 107 titles; 77 are listed in the first 2 years and only 30 titles in the last 3 years of the period studied. Biometry and Statistics is next in order with 104 titles which seem to exhibit no regular tendency of frequency. For this category the maximum is in 1933 and the next highest number of titles is in 1936.

The remaining 3 categories have less than 100 titles each during the five-year period. Feeling and Emotion has 83 titles with a maximum in 1933 and a decline since that time, with only 9 titles listed in the last year. Under Nervous System are listed 79 titles with a steady increase each year during this period. Last in rank order is Evolution and Heredity with only 40 titles during the five-year period. The trend is irregular and 17, or nearly half of the total number of titles, appear in the last year.

From these data it would appear that only 3 categories—Child, Social Functions of the Individual, and Nervous System—have had a steady increase in the number of titles published during the five-year period. Animal Psychology and Nervous and Mental Diseases had an increase during the first 4 years with a recession during the last year of the period. The fields of Motor Phenomena and Action, Sensation and Perception, Attention, Memory and Thought and Industrial and Personnel Problems each showed a maximum in the middle year of the period with a decline continued during the last 2 years. The high point for Mental Tests, Special Mental Conditions, Personality and Character, and Emotion was in the second year of this period with a more or less steady decline in the number of titles during the last 3 years. And finally the fields of General, Educational, Biometry and Statistics, and Evolution and Heredity seem to be irregular and show no constant trend during the five-year period under consideration.

In Table VII are the results of the number of Members who have published in the different fields. In this table also are indicated the percentage of Members publishing in each field and the rank order. General is, of course, the highest field with 188 Members or 32% publishing. Educational Psychology, Social Functions of the

Individual, Motor Phenomena and Action, and Childhood and Adolescence are the next most popular fields in that order, each with 20% or more of the Members publishing at least once during the five-year period.

Of special interest is a comparison of the results of Plant and Animal Psychology as indicated in Tables VI and VII. This category is in second rank order in Table VI with a total of 438 titles—a very close second to General Psychology. In Table VII it drops to seventh rank order with only 94 Members contributing to the field—

TABLE VII
NUMBER OF MEMBERS PUBLISHING IN THE DIFFERENT FIELDS

Field	Number of Members Publishing	Percentage of Members Publishing	Rank Order
General	188	32	1
Sensation and Perception	95	16	6
Feeling and Emotion	41	7	14
Attention, Memory and Thought	86	15	8
Nervous System	26	4	16½
Motor Phenomena and Action	142	24	4
Plant and Animal	94	16	7
Evolution and Heredity	26	4	16½
Special Mental Conditions	74	13	11
Nervous and Mental Disorders	75	13	10
Personality and Character	65	11	13
Social Functions of the Individual	150	26	3
Industrial and Personnel Problems	66	11	12
Educational	157	27	2
Biometry and Statistics	40	7	15
Mental Tests	81	14	9
Childhood and Adolescence	117	20	5

a number of contributing Members smaller than those to the fields of General, Educational, Social Functions of the Individual, Motor Phenomena and Action, Childhood and Adolescence, and Sensation and Perception. This comparison indicates that, although fewer Members are interested in this field, their productivity is relatively greater than that of those interested in some of the other fields.

It is also of interest to note that only 40 Members or 7% contributed in the field of Biometry and Statistics during the five-year period, important as this field has become in the treatment of psychological results. Also the important field of Feeling and Emotion is low, with only 41 Members or 7% contributing. At the lowest end of the distribution, only 26 Members or 4% contributed to each of the fields of Nervous System and Evolution and Heredity during the five-year period.

Table VIII records the results of the spread of research interest as indicated by publications in different fields by the same individual during the years 1932-1936. It will be observed that more than half of the Members either published not at all or in only one or two fields. On the other hand, 19% of the Members published in five or more of the fields represented by the *Abstracts'* categories—one individual having publications in 11 of the 17 fields listed in this journal.

A comparison is interesting between what the Members indicate as their research interests (cf. the seventh, eighth and ninth columns of Table III) and their actual publications (cf. Table VI). Unfortunately a direct comparison is not possible because the categories

TABLE VIII
NUMBER OF FIELDS IN WHICH MEMBERS PUBLISHED

Number of Fields	Number of Members	Percentage of Members
0	126	22
1	99	17
2	94	16
3	86	15
4	72	12
5	41	7
6	33	6
7	18	3
8	7	1
9	6	1
10	4	1
11	1	0

adopted by the *Abstracts* are not followed by the fields of interest indicated by the Members in the *Yearbook* of the Association. However, certain groupings of interests would seem not out of place as a basis for such a comparison. The results in Table VII of the number of Members contributing in the different fields as indicated by the categories of classification of the *Abstracts* is also not without interest in this connection.

A few direct comparisons seem possible. For example, 145 Members indicated a research interest in Tests which ranked second in interests, while the actual production of test papers was only 146 during the five-year period and these were produced by only 81 Members. In Childhood and Adolescence the production is well ahead of the expectation. In this field, the rank order is fifteenth in the interests list with only 33 members indicating an interest, while 117 individuals contributed 366 papers according to the *Abstracts'* classification to raise this field to fifth rank order in actual production.

In contrast to this situation, only 123 Members indicated a research interest in Educational Psychology, while 157 individuals produced 402 papers in this field. In both the interests list and the production list Educational Psychology is third in rank order.

An even greater difference in favor of production is seen in the field of the Social Functions of the Individual, for which only 78 Members reported a research interest while 150 Members actually produced 360 titles. A similar situation is found in the field of Animal Psychology with only 42 Members reporting a research interest (eleventh in rank order of interests), while 94 or more than double this number produced 438 titles (second in rank order of the number of titles). It has been pointed out above that there is a greater average publication in this field than in any other.

From this point on direct comparisons fail between interests and publications. It is true that Industrial Psychology appears in both the interests and the publications lists. But Vocational Psychology also appears in the interests list in even greater numbers than Industrial Psychology. A sorting of the records indicates that 64 Members indicated an interest in either Industrial or Vocational Psychology or both (including a few cases of Vocational Guidance and Vocational Tests). Of these 64 individuals, 21 or almost one-third published nothing at all during the five-year period. Probably some of the publications of those listed under Vocational interests appeared in the category of Educational Psychology in the *Abstracts*. Compared to this situation, Tables VI and VII indicate 169 Industrial papers produced by 66 individuals.

The Experimental field is particularly difficult to evaluate because of the number of categories in the *Abstracts* in which Experimental papers may appear. Some few might appear in General, as apparatus notes. Also some few of the titles listed under Nervous System are probably Experimental in character. Animal psychology is almost entirely experimental but has already been considered inasmuch as the term so frequently appears in both the interests and the publications lists. But certainly the categories of Motor Phenomena and Action, Sensation and Perception, Attention, Memory and Thought, and Feeling and Emotion are almost exclusively comprised of experimental papers, although a small number of purely theoretical studies may also appear in these categories. And these 4 classifications—which must be almost totally experimental—give a grand total of 1,009 papers over the five-year period, or more than 25% of the total of 3,963 papers published in all fields. And it will be found, in

Table III, that 35% of the Members in 1937 indicated Experimental as one of their fields of research. After all "Experimental" is a method and not a field of psychology, and hence the comparison of interests and actual publications is difficult.

Even more difficult to estimate is the production of what should be classed under Clinical Psychology, because this is both method and/or point of view or a field of research in the minds of different psychologists. In the present study, the following categories of classification of titles in the *Abstracts* can rather safely be eliminated from the discussion—Educational, Social Functions of the Individual, Industrial and Personnel Problems, and Mental Tests, because these topics appear with great frequency also in the interests table and have been treated separately above. One may eliminate these categories for the same reason that Animal Psychology was eliminated above. Some small part of Educational Psychology might be considered Clinical as well as some very small part of the Social Functions of the Individual. Many psychologists would include all of the titles under Industrial and Personnel Problems in the Clinical field, and certainly many of these papers should be so assigned. Some psychologists would include all of Mental Tests in the Clinical field, but the great frequency with which Mental Tests or Tests appears separately in the interests list alongside of Clinical Psychology should give the basis for the elimination of this topic from the strictly Clinical field.

This leaves 5 of the *Abstracts'* categories to be included in the field of Clinical Psychology, in part at least. Some part of Childhood and Adolescence, of Nervous and Mental Diseases (although most of this would seem to be Abnormal), of Personality and Character, of Special Mental Conditions, and of Heredity and Evolution should be included under the bracket of Clinical, but how much of each simply cannot be estimated. If we consider these 5 categories and add both Industrial and Personnel Problems and Mental Tests, we obtain a grand total of 1,118 titles which were published during the period 1932-1936. If every title of these 7 categories is to be considered Clinical Psychology, this represents 28% of the total number of titles published during the five-year period. If less than 100% of these 7 categories are to be considered Clinical Psychology the following percentages of the total number of titles are found: if 75%—21% of the titles; if 50%—14% of the titles; and if 25%—only 7% of the titles are to be considered Clinical. The writer frankly is unable to interpret or even guess the correct percentage of the titles in these 7 categories which should be considered Clinical

Psychology, and so leaves the interpretation to the reader. But it may be pointed out that 18% of the 1937 Members of the Association reported Clinical as a special field of research interest. There were 254 Members or 43% of the total membership who reported one or more of the following groups as a research interest, namely, Clinical Guidance, Industrial, Mental Hygiene, Personnel, Tests, and Vocational.

One further matter seems worth discussing, the relative prevalence of publication of Members in academic and non-academic positions. If the summary in the *Yearbook* did not mention any academic connection whatsoever that individual was classed in the non-academic group. The type of position held, as indicated in the 1937 *Yearbook* of the Association, was considered alone as the basis for the classification, and the fact that an individual may have changed from an academic to a non-academic position during the five-year publication period or vice-versa was disregarded. In Table IX will

TABLE IX
NUMBER OF TITLES OF MEMBERS
ACADEMIC AND NON-ACADEMIC POSITIONS

Position	Number of Members	Number of Titles	Average Number of Titles
Academic.....	453	3,545	7.8
Non-academic.....	134	418	3.1
Total.....	587	3,963	6.7

be found the summaries of this analysis. It has been pointed out above that the total 587 Membership published a total of 3,963 titles during the period 1932-1936 or an average of 6.7 titles per person. The 453 Members holding academic positions in 1937 published 3,545 titles or an average of 7.8 titles per individual Member. Contrasted with this, the 134 Members holding non-academic positions published only 418 titles during the same period or an average of only 3.1 titles per individual. In other words, on the average, the academic group was two and one-half times as productive as the non-academic group.

These differences were so striking that it seemed worth while to analyze the situation in more detail. In Table X will be found the frequency of publication of those individuals in non-academic positions. A total of 53 non-academic individuals or 39.6% failed to publish at all as against 73 or 16.1% of those engaged in academic work. Of the non-academic group only 29 individuals or only

21.6% published 6 papers or more in the five-year period. And it will be remembered that 6.7 titles was the average number for the total Membership, both academic and non-academic. The average of the academic group is 7.8 titles, and only 17 of the non-academic group or 12.7% wrote 8 papers or more. Also one individual in the

TABLE X

FREQUENCY OF PUBLICATION

NON-ACADEMIC

Number of Papers	Number of Members	Per Cent of Members
0	53	39.6
1	19	14.2
2	15	11.2
3	11	8.2
4	5	3.7
5	2	1.5
6	4	3.0
7	8	6.0
8	3	2.2
9	3	2.2
10	2	1.5
11	1	0.7
12	1	0.7
13	1	0.7
14	2	1.5
15	1	0.7
16	1	0.7
19	1	0.7
43	1	0.7

non-academic group produced 43 titles, but he is the director of a research laboratory and spends much of his time solely in research. Except for this individual, the largest number of papers produced by anyone in a non-academic position was 19 titles, while 41 Members or 9.1% of the academic group exceeded in frequency of publication the next to the highest of the non-academic group. Also, of the 2 individuals of the non-academic group, one who produced 14

TABLE XI

CONSTANCY OF PUBLICATION

NON-ACADEMIC

Years	Number of Members	Per Cent of Members
0	53	39.6
1	23	17.1
2	20	15.0
3	11	8.2
4	19	14.2
5	8	6.0

TABLE XII
NUMBER OF TITLES BY FIELDS
ACADEMIC AND NON-ACADEMIC

Field	Non-academic			Academic		
	Number of Titles	Per Cent of Titles	Rank Order	Number of Titles	Per Cent of Titles	Rank Order
General	30	7.2	5	423	12.0	1
Sensation and Perception.....	17	4.1	12	337	9.5	6
Feeling and Emotion.....	3	0.7	15	80	2.3	15
Attention, Memory and Thought.....	3	0.7	15	189	5.3	8
Nervous System	1	0.2	17	78	2.2	16
Motor Phenomena and Action.....	18	4.3	11	362	10.2	4
Plant and Animal.....	34	8.1	4	404	11.4	2
Evolution and Heredity.....	9	2.2	14	31	0.9	17
Special Mental Conditions.....	17	4.1	12	90	2.5	13
Nervous and Mental Disorders.....	60	14.4	1	98	2.5	12
Personality and Character.....	21	5.0	9	111	3.1	11
Social Functions of the Individual.....	55	13.2	2	305	8.6	7
Industrial and Personnel Problems.....	25	6.0	8	144	4.1	9
Educational.....	46	11.0	3	356	10.3	3
Biometry and Statistics.....	21	5.0	9	83	2.3	14
Mental Tests	30	7.2	5	116	3.3	10
Childhood and Adolescence.....	28	6.7	7	338	9.5	5

papers and one with 15 papers, both held academic positions until the year before that covered in this study. And these 2 individuals account for 19 of the Animal papers to be discussed later in Table XII.

The problem of constancy of publication for the non-academic group is considered in Table XI. Of the 134 Members in non-academic positions, 53 or 39.6% published nothing at all during the five-year period as compared with 16.1% of the academic group or 22% of the total Membership. Only 8 non-academic Members or 6% of this group published in each of the 5 years as against 122 or 25.2% of the academic group. The entire picture of constancy of publication is very markedly in favor of the academic Members.

Finally, the distribution of publications by *Abstracts'* categories, for both the academic and non-academic groups, will be found in Table XII. This table consists of a breaking down of the total results already reported in Table VII above. In this table are given the number of titles, percentage of titles, and rank orders for both the academic and non-academic groups. We shall consider only rank orders as an indication of enough research interest to lead to publication in the present discussion. If we set a criterion of a change of at least 3 places in rank order, the following relations are evident. The same amount of publication interest, as measured by this criterion, is found for the academic and non-academic Members in the following fields: high in both groups—Animal¹ and Educational; same and median in both groups—Personality and Character, Industrial and Personnel Problems, and Childhood and Adolescence; same and low in both groups—Feeling and Emotion, Nervous System, Evolution and Heredity, and Special Mental Conditions. The fields in which the non-academic group are lower in rank order than the academic in an amount greater than 3 ranks are General, Sensation and Perception, Attention, Memory and Thought, and Motor Phenomena and Action. In the last 3 fields, the differences are great—greatest in that of Motor Phenomena and Action, where the rank order from academic to non-academic groups drops from fourth to eleventh place. There is a slightly increased interest, evidenced by the rank orders of the non-academic and academic groups in favor of the former, for Biometry and Statistics, and a marked change in rank order in the same direction for Social Functions of the Individ-

¹ We have indicated, however, in the discussion of Table X that 19 of the 34 Animal papers in the non-academic group were produced by 2 individuals while in previously held academic positions.

ual and especially for Nervous and Mental Disorders, which rises from twelfth rank for the academic group to first place in the non-academic.

From a consideration of these results, it becomes obvious that the non-academic Members are less interested in research in those fields which require laboratory technique and which lead to results of more theoretical than practical importance, while they are more interested in those fields concerned with the practical relation of the individual to his environment or to his fellow man, and these fields are best approached by the clinical and test methods.

Certain rather obvious conclusions may be drawn from this study.

1. During the 20 years between 1918 and 1937 there has been a shift of interest from theoretical and academic problems to clinical and practical problems—a shift of emphasis which is still further evident when one compares the results of the older Members with the younger and less mature Associates in 1937.

2. The study of the actual published research also evidences a similar shift of interest to practical and clinical problems by the non-academic group as compared with the academic group or the Membership as a whole. These results are clear-cut when one considers the total publications of the Membership over a five-year period (1932-1936).

3. In actual number of titles published Animal and Educational Psychology and Motor Phenomena and Action all rank high for the total Membership while Evolution and Heredity, Nervous System, and Feeling and Emotion rank low.

4. The non-academic group are markedly less productive than the academic group in the matter of published research.

A SUMMARY AND EVALUATION OF ALTERNATIVE PROCEDURES FOR THE CONSTRUCTION OF VINCENT CURVES

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Vincent in 1912 (12) proposed a procedure for averaging individual curves in order to reveal the form of the learning function when learning is carried to a criterion of mastery. Her method for obtaining a composite curve eliminates many of the variations due to individual differences in rate of mastery. For many purposes these differences are of major interest, and in such cases the Vincent curve is not a good instrument for presenting the learning data. The one and only use for which the Vincent curve can be recommended is to reveal the *form* of the learning function. This is a limited use, but one of some importance.

Vincent applied her method to maze data. She divided the number of trials to mastery by 10, and then averaged the time or errors per trial per tenth of trials.¹ In order to avoid fractions, she divided the total trials into 10 whole numbers, and then distributed the excess trials over the first groups. Thus the 10 trial divisions of an animal requiring 22 trials would be 3,3,2,2,2,2,2,2,2,2. That is, 10 groups of 2 trials each leaves an additional 2 trials to be distributed among the first trial groups, so that the first and second 'tenth' of trials contain 3 trials each. The average score of the first 3 trials was the first point on her curve, the average of the second 3 the second point, the average of the next 2 the third point, and so on. Her method may be characterized as approximating a curve of average errors per trial per tenth of trials to mastery. The arbitrary distribution of the excess trials led later experimenters to abandon Vincent's actual procedure.

Modifications deriving from Vincent's procedure have been used widely in reporting average results from maze experiments, memori-

¹ The use of 10 divisions is, of course, arbitrary. To avoid circumlocution, division into tenths will be described throughout the discussion, although any other fraction of trials may be used. The question of appropriate number of divisions is considered later.

zation experiments, and conditioned response experiments. Of the many procedures now in use none corresponds exactly to that of Vincent. The various procedures arrive at numerically different values from the same data, weight individual performances differently, introduce different artifacts into the resulting composite curves. Because of the confusion which exists, it seems desirable to assemble in one place the chief methods used and to subject them to a logical analysis in order to make recommendations of the procedures which best serve the purposes for which the Vincent curve is intended.

THE FUNDAMENTAL REVISIONS: HUNTER AND KJERSTAD

Hunter was one of the first to use the Vincent curve (6) and he has used it extensively since. His procedure is described in detail in a review of learning experiments (5). Eliminating the criterial trials, he divides the remaining trials into tenths. He then assigns the errors made in each trial to the tenth of total trials to which they belong, interpolating as necessary for fractions of trials by assuming that trials are continuous and errors uniformly distributed within the trial. That is, if a tenth of trials includes 2.2 trials, the first point on the Vincent curve will include all of the errors of the first 2 trials plus 0.2 of the errors of the third trial. Hunter's final curve may be characterized as one of total errors per tenth of trials to mastery. It differs from Vincent's in 2 important respects: (1) it makes use of fractions of trials, and (2) it does not convert total errors per tenth to average errors per trial. Hunter's procedure introduces a natural weighting whereby subjects making more total errors prior to the criterion have a greater influence on the form of the average curve than subjects making fewer errors prior to the criterion.

Kjerstad (8) abandoned Vincent's procedure of finding the average score within a given fraction of trials, and substituted instead the score which was reached at the end of a given fraction of trials. The common assumption that Kjerstad's method is simply a graphical device for securing Vincent's values is evidently erroneous. Kjerstad applied his procedure to memorization scores, and it has become the standard device for summarizing such data. As he used it, the curve showed scores per trial at the end of each fraction of trials to mastery. Because the Kjerstad scores come at the end of trial units, rather than at the average, there are 11 values for tenths of trials, including an initial score and one at mastery. Thus the major differences between Kjerstad's procedure and Vincent's are 3: (1) the score of the first trial determines a first point, but it is not averaged into

tenths of trials; (2) not all of the data are used, but only those values found by interpolation at the end of tenths of trials; (3) the criterial trial alone determines the final value.

Bills (1, p. 196), in adapting Kjerstad's method to error scores, treats the first trial differently from Kjerstad. He assumes that the first obtained score counts as a measure of learning, instead of representing a baseline from which improvement is made, and his graph is therefore indeterminate at trial 0.² As a consequence, the Bills procedure runs into difficulty if it is necessary to interpolate for a fraction less than one trial. Thus if 8 trials are to be divided into tenths, the first score must be interpolated at 0.8 the distance between trial 0 and trial 1. Since there are no real scores at zero, the interpolation is ambiguous. It is probably undesirable to interpolate for fractions less than one trial, however, so that this drawback is not serious. It represents a change from Kjerstad's practice, however, and results in 10 values instead of the 11 which the original Kjerstad procedure gives. The final value is that of the last trial counted, usually the criterion.

Since other discussions of the Vincent curve take as their point of departure either Hunter's procedure or Kjerstad's, the differences between the methods are of some importance. In Table 1 are summarized specimen data treated by the 2 methods. In order not to emphasize non-essential differences, the Bills modification of the Kjerstad method has been used, since, like Hunter's procedure, it results in 10 values. Also the criterial trials have been omitted in both methods so that exactly the same number of trials and the same data are subjected to treatment by both. Several points from Table 1 deserve attention. It will be noted that the Hunter values retain the same total errors as in the raw trials, so that values of greater size enter the composite from A than from B. The Kjerstad curve utilizes from A only the data of every other trial. It thus reduces the reliability of the scores by using only half of the data. It will be seen also that the total scores for A and B by the Kjerstad procedure

² The Bills modification of Kjerstad's practice is not immediately obvious because it is customary to count the errors in the first trial of maze learning as part of the series, while the initial trial in memorization is often counted as a preliminary exposure, the results of which are really measured on the next trial. If it were the custom to count errors rather than the correct responses in memorization experiments, the difference between Bills' and Kjerstad's practice would be apparent immediately. Kjerstad's practice of counting the first trial as a baseline from which improvement is made is probably preferable for maze scores as well as for memorization scores. In this connection, see Ghiselli (2).

are more nearly alike than the Hunter totals. The Kjerstad values are scores-per-trial which tend to be more consistent from one subject to another than scores-per-tenth-of-trials as used by Hunter. It is evident that these differences will be reflected in the composite, determined by averaging the individual curves. The reversal in the

TABLE 1

VALUES FOR POINTS ON VINCENT CURVES AS CALCULATED BY HUNTER
AND BILLS-KJERSTAD PROCEDURES

Two Sets of Raw Scores; Errors Per Trial			Hunter Procedure			Bills-Kjerstad Procedure		
A (20 Trials to Mastery)		B (11 Trials to Mastery)						
			A	B	Mean	A	B	Mean
(1)	36	(1) 20	(1) 60.0	21.0	40.5	24.0	19.0	21.5
(2)	24	(2) 10	(2) 43.0	10.8	26.9	20.0	9.8	14.9
(3)	23	(3) 9	(3) 35.0	9.3	22.2	16.0	8.4	12.2
(4)	20	(4) 7	(4) 22.0	8.1	15.1	9.0	7.4	8.2
(5)	19	(5) 8	(5) 17.0	7.3	12.2	9.0	6.5	7.8
(6)	16	(6) 5	(6) 12.0	6.1	9.1	5.0	5.6	5.3
(7)	13	(7) 6	(7) 8.0	10.1	9.1	4.0	9.5	6.8
(8)	9	(8) 11	(8) 2.0	5.7	3.9	1.0	4.6	2.8
(9)	8	(9) 3	(9) 2.0	2.4	2.2	1.0	2.1	1.6
(10)	9	(10) 2	(10) 2.0	1.2	1.6	1.0	1.0	1.0
(11)	7	(11) 1						
(12)	5	(12) 0						
(13)	4							
(14)	4							
(15)	1							
(16)	1							
(17)	1							
(18)	1							
(19)	1							
(20)	1							
(21)	0							
Total Error Score	203	82	203.0	82.0	142.8	90.0	73.9	82.1

seventh tenth of practice is emphasized in the Kjerstad curve and smoothed out in the Hunter curve. While such differences may or may not show up from different samples of data, the fact that they may occur points to differences in weights assigned to individual performances by the 2 methods. It is evident also that the variability of points on the composite will tend to be greater by the Hunter method because of the different sizes of scores entering the composite, in spite of the lower reliability of the Kjerstad values.

The values in Table 1 are sufficient evidence that the 2 basic revisions of the Vincent method differ from one another in important

ways, and that each has both advantages and disadvantages. These have been recognized by experimenters who have attempted to use the procedures, and a number of supplementations have been made.

The criticisms and suggestions offered will be interpreted in connection with a discussion of the more general desiderata for an acceptable Vincent procedure.

CONSIDERATIONS DETERMINING THE ACCEPTABILITY OF ALTERNATIVE VINCENT PROCEDURES

The following propositions may be suggested as a basis for evaluating different procedures for the construction of Vincent curves:

1. As much of the data as possible should enter into the computations, in order to achieve maximal reliability.
2. Artifacts due to the criteria of mastery should be eliminated. It is important that the form of the curve and the variability of each point should be determined by individual performances, and should not be artifacts produced by the scoring device.
3. Individual performances should be weighted systematically before entering into the determination of the composite curve.

Each of these propositions will be discussed in turn.

1. *Utilization of Data.* The Kjerstad method and its derivatives are less satisfactory than the Hunter method and its derivatives with respect to the utilization of data. The interpolation procedure of Kjerstad uses only portions of the data. This is especially evident when total trials are an even number of the fractions on which the curve is based. Thus a thirty-trial series divided into tenths will be represented only by the score of every third trial. The difficulty arises in all cases in which the number of trials is equal to or greater than twice the number of fractions into which the trials are divided.

2. *Artifacts Due to Criteria.* Melton (10) has pointed out an artificial end-spurt which occurs in the Kjerstad method when the criterial trials are included in constructing the curve. The artifact is found also in Hunter's method if the criterial trials are included. In a recent monograph Ward (13) has shown clearly that a control trial following the reaching of a given criterion in memorization tends to yield scores below the criterial value.

Melton proposed a revision of the Kjerstad procedure to eliminate the artifact at the criterion without discarding the information given by the approach to the criterion. He solved the problem by reversing the dependent and independent variables in determining

his points. That is, instead of charting the scores at successive tenths of trials, he charted the trials required to achieve specified increases in score. The actual practice was to plot the scores as Kjerstad does, then to read the interpolated values from the other axis. A comparison of the Kjerstad and Melton methods is made in Figure 1, in which are plotted curves from Melton's data. The final end-spurt of the Kjerstad curve is evident in the figure. It is clear also that the Melton curve does not show the final spurt. The Melton curve lies above the Kjerstad curve throughout the later portions until the final point. What this means is that after the first third of trials the memorization curves tend to fluctuate down as well as up. Melton's curve is influenced only by the upward fluctuations, since his points are determined by the *first* time that a given value is reached. Kjerstad's is influenced by the downward fluctuations as well, since his points are determined by the level of performance at a given fraction of trials, regardless of how high the performance may have been previously. Only at the criterion does the Kjerstad procedure select an artificially high point.

The Melton curve, as plotted in Figure 1, shows initial positive acceleration, which Melton believes to be an artifact.³ While his

³ Melton gives an example (10, n. 5) to show how the artifact arises, but it is somewhat difficult to indicate in words the factors which produce it. The exposition requires this long footnote.

The Melton curve of trials required per unit of mastery should be plotted with units of mastery as the abscissa and trials required as the ordinate. The axes have been interchanged in Figure 1, in order to make direct comparison with the Kjerstad curve. When plotted in its original form, the Melton curve is positively accelerated, that is, is concave upward. The artifact in that case is an initial negative acceleration, out of harmony with the consistent positive acceleration throughout most of the curve. The problem is to explain the negative acceleration in the original curve, which corresponds to the initial positive acceleration in the curve as inverted in Figure 1.

The most frequent raw scores of the first 2 trials in the memorization of short lists, say of 10 items, will be scores such as 0, 1, or 0, 2, or 0, 3. When these are converted to Melton scores of trials required per unit anticipated, the series become 0, 1; 0, 0.5, 1; 0, .33, .67, 1.00. The first series has too few points to describe acceleration, but the second and third series are linear, that is, of zero acceleration. The strongest tendency of Melton scores is towards initial zero acceleration, because of the assumption of linearity made in interpolating for fractions of trials. Since this is true, it is evident that an average result will be accelerated positively or negatively depending upon the prevalence of a few cases with pronounced acceleration. A single positively or negatively accelerated case will make the whole series of average values either positive or negative, if that series would otherwise be linear. The problem reduces

procedure corrects the Kjerstad curve near the criterion, it introduces some difficulties at the beginning of memorization. The logic of the Melton procedure makes it less sensitive to downward fluctuations than to upward ones. In this respect it does not reflect equally the data from every trial.

The artifact at the criterion is found in all methods (except Melton's) if the criterial trials are included in the determination of points on the Vincent curve. Therefore, in the preferred use of the other methods, the criterial trial should be omitted. Where interest centers primarily in the approach to mastery, and it is desirable to treat the data included in the criterion, the Melton procedure is the one to be recommended.

If the criterion is satisfied in 1 trial, it alone may be omitted, because the scores in all earlier trials are free to fluctuate within the same limits, that is, to a score within 1 unit of the criterion.

When the criterion requires more than one consecutive trial of a given score, it is necessary to eliminate the trial *just prior to the*

itself, therefore, to a demonstration that something in the nature of the scores makes negatively accelerated cases more frequent than positively accelerated ones.

Since linearity is assumed in interpolation, the first portion of the curve can depart from linearity only when the score in trial 2 is either 0 or 1. If the score of trial 2 is greater than 1, the scores are linear, as in the examples above. This greatly simplifies the possibilities which need be considered. Negative acceleration (the artifact to be explained) will be contributed by such series if the next trials show gains of more than one item. The series 0, 0, 4 and 0, 1, 4 become in terms of trials required per unit: 0, 1.25, 1.50, 1.75, 2.00, and 0, 1.00, 1.33, 1.67, 2.00. These series are negatively accelerated. Enough cases of this kind, in which initial scores of 0, 0, and of 0, 1, are followed by consistent gains, actually occur to account for negative acceleration. It remains to be shown that positive acceleration is too rare to conceal their influence.

The only cases in which positive acceleration can be produced are score series beginning 0, 1, 0, and 0, 1, 1. For example, the series 0, 1, 0, 4, and 0, 1, 1, 4 become in trials required per unit 0, 1, 2.50, 2.75, 3.00, and 0, 1, 2.33, 2.67, 3.00. These are positively accelerated at first. Since these possibilities are much more restricted than those previously discussed, positive acceleration is less frequent than negative, and the average curve tends to depart from linearity in the direction of negative acceleration, as pointed out by Melton. The positive or negative acceleration of the individual Melton scores is genuine, and not an artifact. The acceleration in the composite may be considered an artifact only because the assumption of linearity for most of the scores prevents their cancelling the effects of the few cases which do, in fact, show pronounced acceleration.

criterion, in addition to the criterial trials. The necessity for this elimination has been discussed by Hilgard and Campbell (3). Suppose, for example, that a maze is to be considered mastered when there are 3 consecutive perfect trials. Then the fourth trial from

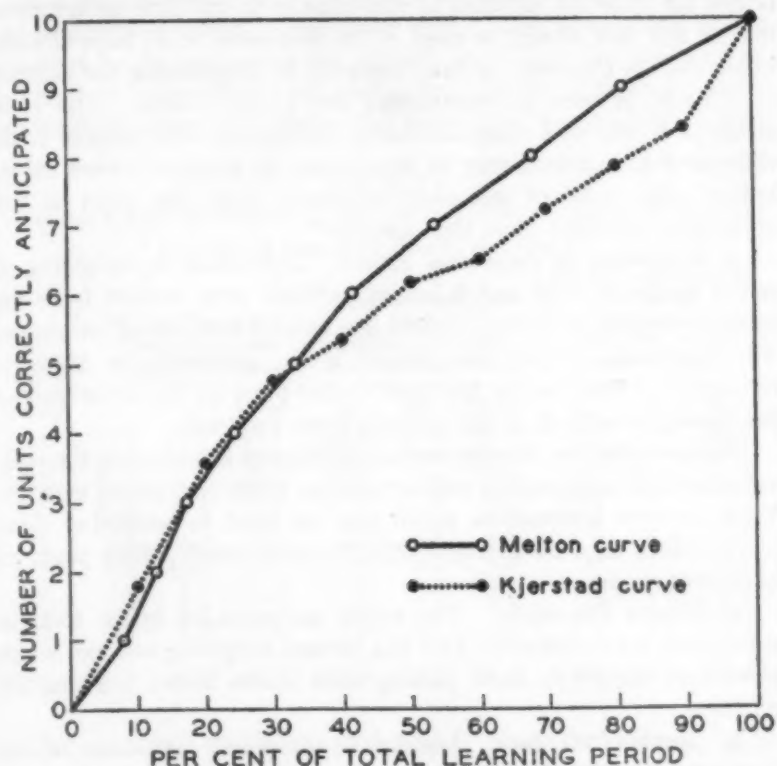


FIG. 1. A COMPARISON OF KJERSTAD AND MELTON VALUES FOR THE SAME MEMORIZATION DATA

The data are from Melton (10), Tables 1 and 4, and were secured from the scores made by 24 subjects in memorizing ten-unit lists of nonsense syllables by the anticipation method to a criterion of 1 errorless trial. The Melton data represent the other regression from the Kjerstad data, and the axes are the reverse of those used by Melton for plotting his results.

the end must of necessity contain at least 1 error for each subject, whereas the fifth trial from the end may be without error. The trial just before the criterion has therefore a special restriction imposed on it. In the case of a memorization experiment, suppose the criterion to be 2 successive perfect recitations of a list. Then the trial third from the end (just prior to the criterion) must have a value at least

1 unit below mastery. If the artificially restricted trial just prior to the criterion is permitted to enter into the averages, the final score will show effects of these restrictions.

The elimination of criterial trials, and in some cases the trial just before the criterion, appears to run counter to our first proposition, that all the data should be used. The eliminated trials have already influenced the treatment of data, however, by determining the number of trials to be used in constructing the Vincent curve. The variability of points on a composite curve constructed with criterial trials eliminated as indicated may be determined by straightforward calculations with standard statistical measures, since one point is not artificially restricted more than another.⁴

3. *Weighting of Individual Scores.* Differences in weighting of scores by the Hunter and Kjerstad methods were evident from the values presented in Table 1. Both are natural methods of weighting. The importance of any one subject in the composite by Hunter's method is determined by his total scores prior to the criterion; by the Kjerstad method, by his average score per trial.

Suppose that the Hunter method is adopted for securing the individual scores expressed in total errors per tenth of trials to mastery. What are the alternatives which may be used in weighting these scores before they are averaged into the composite? Three practices have been proposed, as follows:

a. *Hunter Procedure.* The scores are averaged in the form in which they were obtained. This is a natural weighting method, which gives most weight to those making most scores before reaching the criterion.

b. *Loucks Procedure.* Loucks (9) proposed a revision of the Hunter method which is, except for Vincent's arbitrary trial divisions, a return to Vincent's original procedure. He suggests dividing the Hunter scores by the number of trials entering into each tenth of trials, thus arriving at scores expressed as average errors per trial per tenth of trials to mastery. Dividing each score by a constant, representing the number of trials entering that score, is to introduce another type of natural weight. It makes scores somewhat more com-

⁴ The limit at mastery affects the variability of scores which approach the limit, while not affecting scores which do not approach it. Any scoring device which permits a finite limit to be reached runs into statistical difficulties in determining the dispersion of values near the limit. Scores obtained after the elimination of criterial trials are not actually free of this difficulty, and simple probability theory does not strictly apply.

parable from individual to individual than Hunter's raw scores. The Loucks values and the Bills-Kjerstad values are very similar, except that the Loucks scores are based on all of the data.

c. *Hilgard and Campbell Procedure.* A method proposed by Hilgard and Campbell (3) introduces a definite weighting, based on statistical considerations.⁸ Beginning with Hunter scores, they divide each score by the total score and multiply by 100, so that the score in each tenth is indicated as a per cent of total scores to mastery. Each individual therefore contributes the same total score (100%) dis-

TABLE 2
VALUES FOR POINTS ON VINCENT CURVES AS WEIGHTED BY DIFFERENT METHODS
(Calculated from Raw Scores of Table 1)

Tenths of Trials	Hunter Procedure (Total Errors per Tenth of Trials)			Loucks Procedure (Average Errors per Trial per Tenth of Trials)			Hilgard and Campbell Procedure (Per Cent of Total Errors per Tenth of Trials)		
	A	B	Mean	A	B	Mean	A	B	Mean
(1)	60.0	21.0	40.5	30.0	19.1	24.6	29.6	25.6	27.6
(2)	43.0	10.8	26.9	21.5	9.8	15.7	21.2	13.2	17.2
(3)	35.0	9.3	22.2	17.5	8.4	13.0	17.2	11.3	14.3
(4)	22.0	8.1	15.1	11.0	7.4	9.2	10.8	9.9	10.4
(5)	17.0	7.3	12.2	8.5	6.6	7.6	8.4	8.9	8.7
(6)	12.0	6.1	9.1	6.0	5.5	5.8	5.9	7.4	6.7
(7)	8.0	10.1	9.1	4.0	9.2	6.6	3.9	12.3	8.1
(8)	2.0	5.7	3.9	1.0	5.2	3.1	1.0	7.0	4.0
(9)	2.0	2.4	2.2	1.0	2.2	1.6	1.0	2.9	2.0
(10)	2.0	1.2	1.6	1.0	1.1	1.1	1.0	1.5	1.3
Total Scores	203.0	82.0	142.8	101.5	74.5	88.3	100.0	100.0	100.3

tributed over the 10 fractions of trials. Since the Vincent curve desired is one of relatively pure form, as free as possible from influence by the units of measurement, it seems reasonable to consider total scores as 100% just as total trials to mastery are considered as 100%.

In order to clarify the differences which result from the use of the different methods of weighting, scores from the example in Table 1 are presented in Table 2 as weighted by the different methods. It is clear that the total scores differ from one method to the other, and the discrepancy between the scores for A and B likewise differs. The natural weighting procedure of Loucks reduces the discrepancy more than that of Hunter, but only the Hilgard and Campbell pro-

⁸ The method was earlier used by Hunter (4) for a few comparisons.

cedure deliberately removes the discrepancy in totals by the practice of artificial weighting. Weighting procedures in addition to those mentioned may be adopted for special purposes.

RECOMMENDED PRACTICES AND SOME PRECAUTIONS

Vincent curves should not be used indiscriminately. Before using the procedure for the presentation of data, an experimenter should ascertain whether or not it is appropriate to his data. Having decided to construct Vincent curves, further decisions are involved in the selection of trial divisions, weightings for individual scores, and computational devices. While considerable latitude exists, some guiding suggestions follow.

(1) *When to Use and When Not to Use Vincent Curves.* Vincent curves should be used only when the form of the practice function is a major interest, and when there is evidence that a composite curve is legitimate. There are situations in which the psychological processes determining scores undergo such important qualitative changes from early to late trials that a composite curve masks the information given by individual curves. Digit-symbol substitution and typewriting are familiar examples of this kind. Renshaw and Schwarzbek (11) believe Vincent procedures to be inapplicable to pursuit tasks.

There are performances in which a decrement commonly occurs after a maximal score is reached. Since this maximum is likely to differ from individual to individual, learning cannot be carried to a meaningful criterion of mastery, and hence the Vincent procedure is inapplicable. While Kantrow (7) has presented 'Vincent' curves of infant conditioning which show a decline beyond their maximum, these cannot be orthodox Vincent curves, since a Vincent curve always ends at a point of mastery.

Whether or not to use Vincent curves is a matter of judgment based on a knowledge of the psychological functions which are being scored. If improvement is relatively continuous from the beginning of practice to the reaching of a criterion of mastery, the Vincent procedure may be applicable. Ordinarily, if plateaus or other qualitative changes are characteristic features of individual curves, the Vincent procedure should not be used without careful check to determine whether the individual features are represented fairly in the composite.

(2) *Should Scores from All Individuals Be Combined?* One advantage of the Vincent procedure is that it makes possible the com-

binning of scores from subjects requiring widely different amounts of practice to reach a criterion. This advantage should not be used, however, without empirical justification. Thus if Vincent curves constructed for a relatively homogeneous group of slow learners differ significantly from curves constructed for a relatively homogeneous group of rapid learners, the scores from slow and rapid learners should not be combined into a meaningless average. Heterogeneous performances cannot be made homogeneous by the magic of a scoring device. The Vincent procedure is still useful in the situation just described, since it reduces the scores of slow and rapid learners to comparable coördinates, where differences in the form of the function can be compared more readily. The range of ability to be represented in a single composite curve may be determined in part through empirical samples. If slow, medium, and rapid learners show Vincent curves insignificantly different, all may be combined.

(3) *How Many Divisions Should Be Used?* Although for expository purposes tenths of trials have been discussed, it is evident that a choice is involved in the number of divisions of trials to be selected.

Since the purpose of the procedure is to reveal the form of the learning curve, it would appear desirable to have as many divisions as possible. Too many divisions, however, will not only reduce the reliability of individual points but also introduce artifacts. The artifact in the first part of the curve by the Melton procedure (cf. footnote 3) arises because of the necessity for dividing the first few trials into too many fractions-of-trials-per-unit-of-score. In the Hunter procedure, a smooth curve may become saltatory if it is necessary to secure more points than there were trials to mastery. For example, a linear series of scores such as 30, 24, 18, 12, 6, if made into 10 scores becomes 15, 15, 12, 12, 9, 9, 6, 6, 3, 3. This converts a straight line into a step-wise curve. *Ordinarily it is undesirable to use more divisions than the trials required by the most rapid subject to reach the criterion.* If the most rapid subject reaches the criterion in too few trials to determine reliably the form of his individual learning curve, it is preferable to omit this subject from the composite rather than to introduce unreliable values into the average curve.

Too few divisions may conceal important changes in the slope of the curve, and make it impossible to locate precisely the inflection points. On the other hand, fewer divisions will result (in derivatives of the Hunter procedure) in more reliably determined values, and presumably in smoother curves. The calculations are, of course, simplified when fewer divisions are used.

Judgment must determine the most desirable divisions for the purposes at hand. Rule-of-thumb recommendations cannot be made because volume of data, precision of measures, grossness of differences, all enter. One does not measure telegraph poles with micrometers, and practice must be suited to the data.

(4) *Which Procedure Should Be Selected?* Having settled the propriety of using Vincent curves, and determined upon the precision of analysis desired, choice of a particular procedure remains. The choice depends upon a number of factors, including the conventions which have been adopted by workers in particular fields. Experimenters in the field of human memorization have preferred Kjerstad's procedure, so that comparisons with the literature are made more conveniently if a closely related procedure is used. Those working with animal mazes have more frequently (though not exclusively) used derivatives of Hunter's procedure. While the criteria discussed in the body of the paper determine in part the choice to be made, there are some further characteristics of each of the procedures which may be considered in their practical selection or rejection.

Hunter's procedure weights disproportionately the subjects making excessive errors before reaching the criterion. Since the curves of such subjects may be atypical, his weighting is seldom to be preferred.

Kjerstad's, Bills', and Loucks' procedures all arrive at a weight determined essentially by the average-score-per-trial. When scores for individual subjects have a common range, these 3 methods are satisfactory, because they weight the subjects approximately equally. This condition is fulfilled, for example, in memorization experiments, in which scores range from zero to mastery for all subjects. It is approximately fulfilled in well-designed mazes in which retracing is prevented, in which scores tend to range from an initial chance value to a final score of zero. The procedures have the desirable characteristic that average scores are expressed in the units of the original data, *i.e.* scores per trial.

It may be recalled that Bills' and Loucks' procedures are most nearly alike, each resulting in 10 scores. Kjerstad's results in 11 scores, including an initial baseline score. Kjerstad's practice is often to be recommended because the first trial is commonly a test of original performance under the experimental conditions rather than a test of improvement. Since the Loucks procedure has the advantage of greater reliability through the use of all of the data, the Loucks procedure could be used with a slight variation to arrive at scores

comparable to Kjerstad's rather than to Bills'. The trials would simply be renumbered, so that trial 1 would become trial 0, trial 2 would become trial 1, and so on. The scores of trial 0 would be averaged as a baseline of chance performance; the remaining scores would be used in determining scores per tenths of trials to mastery. An empirical test would be necessary (in an extensive program) to determine whether the increased reliability of Loucks' values would be worth the added computational effort.

In discrimination experiments, a single trial is often scored as right or wrong. A series of such scores, eventually reaching a criterion, are readily handled by procedures deriving from Hunter, but cannot be treated by the interpolation procedures following Kjerstad unless the scores are first grouped artificially, say into series of 10 trials.

The Hilgard and Campbell procedure is to be preferred when scores from different individuals do not have a common range. This is true of mazes in which retracing is permitted, of problem-situations in which the learner furnishes some of the alternatives, of many performances with work-limit or time-limit scores in which initial performances vary widely even though final scores are brought to a common level. The procedure is applicable to any of the scores which can be treated by the other procedures, but it has the disadvantage for expository purposes of units of proficiency expressed in per cent of total scores, rather than in the units of measurement of the original data. When original units can be preserved without loss, it is desirable to use them.

Whereas each of the other methods uses the regression of score upon trials, Melton's uses the regression of trials upon score. His method may be used with any data, but is preferably confined to situations in which progress towards the criterion is relatively uniform. When there are appreciable regressions to lower scores, it may produce a somewhat distorted picture. It is the only method which determines its division points by scores, in accordance with the same logic by which mastery is determined. For this reason it is the only method which gives a fair picture of the approach to mastery.

(5) *Computation.* Methods of computation are straightforward, once the procedure has been selected. The basic procedures are still those of Hunter (5) and Kjerstad (8). When either is used the criterial trial should be omitted and, if the criterion requires more than one trial, the trial just prior to the criterion should also be omitted. In the Kjerstad procedure the first trial is not counted in

determining the number of trials to be divided into fractions. For practical purposes the first trial in the Kjerstad situation is best handled by renumbering the trials, so that the series begins with trial 0. This is also possible in the other procedures, but the practice has not been followed. The obtained scores should be weighted according to one of the procedures described before the composite is secured.

Several aids to computation have been prepared. Kjerstad interpolated graphically. Loucks used an elastic ruler which he stretched along his scores in order to make a rough graphical division of them. Melton has prepared mimeographed tables useful in plotting scores for interpolation by the Kjerstad procedure. Special forms have been prepared for use in the writer's classes in order to facilitate computations by the procedures deriving from Hunter.*

It does not suffice, in reporting average curves, to state that they are Vincent curves, because of the variety of procedures which may be used in arriving at composite values. If standard procedures are used, they may be named according to those who have described the procedural details. If modifications are made, they should be indicated. The reader should be informed, either directly or by reference to other sources, regarding (a) the nature of the criterion of mastery, (b) the disposal of the first trial, (c) the disposal of the criterial trials, (d) whether the scores represent interpolations between raw scores (Kjerstad *et al.*) or cumulative interpolations (Hunter *et al.*), (e) the procedure by which individual scores were weighted before entering the composite. Many Vincent curves reported in the literature cannot be interpreted accurately because of the lack of relevant information.

SUMMARY

1. The Vincent curve procedure is designed for the averaging of individual performance curves in order to reveal the form of the learning function when learning continues to a criterion of mastery. This is its only legitimate use.

2. The chief revisions of the original Vincent procedure are those of Hunter and of Kjerstad. Hunter's procedure has been supplemented with different methods of weighting individual scores by Loucks and by Hilgard and Campbell. Kjerstad's method has been

* Specimen copies of the mimeographed directions and blank forms used in the Stanford laboratory will be furnished on request to the writer.

modified by Bills and by Melton. Each procedure arrives at different sets of values from the same data.

3. In order to choose between the various alternatives, the following desiderata are suggested: (1) as much of the data as possible should enter into the calculations, thus assuring maximal reliability; (2) artifacts due to the criteria of mastery should be eliminated; and (3) individual performances should be weighted systematically before they enter into the determination of the composite curve. The advantages and disadvantages of the different procedures are considered in the light of these characteristics of a desirable practice. While no one method is indorsed for all purposes, recommendations are made and precautions indicated.

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A CONSIDERATION OF THE USE OF THE TERM OCULAR DOMINANCE

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The literature contains numerous and varied uses of the term "dominant eye." While these uses differ as to the methods of determining the relationship between the eyes and in interpretations of the relationship, there is a common assumption that in normal binocular functioning one eye dominates over the other. It is with the assumption of such a unitary factor that this paper is concerned. The dominance is assumed to be evident in the greater use of one eye, in greater sensitivity, in lead movements, or in greater preference for one eye whenever a choice must be made between them.

Implications have ranged from the assumption that laterality of manual functions is determined by eye dominance (Parson, 23) and that eyedness is an indication of central laterality (Jasper, 16; Orton, 21), to the belief that methods of teaching reading should be influenced by the factor (Orton, 21, 22). The agreement between so-called dominant hand and dominant eye has been described as preferable to a mixed laterality of these 2 functions (Oates, 20; Dearborne, 9).

While each use of the term "ocular dominance" commonly implies that the characteristic is a unitary factor, there are many methods of determining it. If the unitary factor exists, however, each test should indicate the same laterality, or if there is lack of agreement, it is presumably implied that some tests are not good measures of eye dominance. The lack of agreement may be an indication, on the other hand, that ocular dominance is not a general, unitary factor; that it is specific to the method of measurement, and to the situation in which the test is made. Evidence from experimental measurement and from ocular neurology and physiology is to be presented to indicate that a general eye dominance as commonly conceived does not exist. It is not the purpose of this paper to review

the literature in detail but, rather, to cite representative studies. Reviews of the field have appeared in recent publications (18, 25).

EXPERIMENTAL EVIDENCE

The most common tests devised to indicate "eye dominance" have been various kinds of sighting procedures. These tests measure a preference for one eye over the other in situations where only one eye can be used. Cone-shaped sighting devices, peep-hole sighting, and pointing methods are examples of such procedures. Four points of criticism of these methods as indications of a general factor of eye dominance may be listed.

(1) There is no justification for the belief that the "sighting eye" is preferred, dominates, or assumes leadership in normal binocular vision, although such a belief is often expressed. On the contrary, some recent experimental evidence indicates that during "normal" stereoscopic vision, simple interfixation movements, and convergent and divergent movements, neither eye tends to assume leadership.

Clark (3) reported that, while one eye moved first from 75% to 88% of the time, there was no significant relationship with the sighting eye as measured by the V-scope method (19). In a study of the time required to complete divergence movements during fixation in reading (2), it was found that there was no significant difference in 60% of the cases. In 25% the sighting eye (Miles V-scope) completed the movement first and in 15% the movement was completed first by the "non-dominant" eye. Clark (4) in an investigation of convergence and accommodative convergence found no significant difference between the behavior of the "dominant" and "non-dominant" eyes (Miles V-scope) for a variety of factors considered. Jasper (16), on the other hand, reported a somewhat greater tendency for the left eye to move first in righthanded subjects and the right eye to move first in lefthanded individuals. However, Jasper's data were not obtained during "normal" binocular functioning so they are not directly comparable. It should be pointed out that eye-movement studies can give only indirect evidence in regard to dominance in visual *perception*.

There is also evidence (3) that, due to eye-movement during stereoscopic vision, clear vision shifts from one eye to the other with no dominance evidenced. Washburn (28) has pointed out the importance of shifting perception from one eye to the other in stereoscopic vision.

(2) The agreement between different "sighting" tests, while positive, is not high enough to insure that all are measuring a unitary factor. Even relatively similar methods frequently give differing preferences. Variations in situation and procedure change the results. Thompson (26) used 5 different "sighting" tests in a study of 86 subjects. In no case was there perfect agreement between any 2 tests, the percentage of agreement ranging from 60 to 81.

Buxton and Crosland (1) used 4 tests of sighting with 86 subjects and found that, while the reliability quotient of each was from .87 to .98, the intercorrelations varied from .44 to .71, only 2 being above .46. They conclude that "the existence of a 'unitary' trait of eye-preference is not indicated."

(3) Research on sighting by Crider (7) has indicated that, with rigidly controlled criteria and a large number of measurements, the consistency of choice of one eye is diminished and the proportion of "ambiocular" individuals increases. Whereas 93.34% of subjects used one eye consistently in 2 trials, only 50.45% used the same eye throughout 45 trials.

(4) The agreement between "sighting" tests and other so-called "eyedness" tests is little better than chance. In the study already mentioned, Thompson (26) found the "sighting" tests in agreement with convergence and chromatic tests for from 45% to 62% of the cases. Washburn, Faison, and Scott (27) found that the preferred eye in sighting, using the Miles V-scope, was the more responsive eye in retinal rivalry in only 33% of 57 cases. Visual acuity has been found to have no relationship to "sighting eye," or to other measures intended to show dominance. Moreover, the proportion of right-eye and left-eye superiority is a matter of chance. Of Thompson's (26) 86 subjects, 43, or 50%, had no measurable superiority of acuity in either eye; 34% had a more acute left eye; and in 16% the right eye was superior. Gahagan (13) using 100 cases of definite preference for right or left eye in peep-hole sighting, found 40% having equal acuity. Of the remaining 60 cases, 45% had greater acuity of the left eye and 55% greater acuity of the right eye. Of these 60 cases having unequal visual acuity, 27 preferred the less acute eye in the sighting test and 33 used the more acute eye. Schoen and Wallace (24), using 8 observers, reported that the relative flicker frequencies for the right and left eye were not reliably different. This was believed to indicate that "retinal events are unrelated to ocular asymmetry."

A relationship has been suggested between muscular imbalance and the "sighting" eye. In a study of 143 pupils having muscle imbalance in one eye, Crider (5) reported that they generally used "the eye having the most efficient musculature" in sighting with the manoptoscope and in Selzer's digit reading stereoscope device. Another study (6) of 257 cases of muscle imbalance confirmed the belief that the eye with muscle deficiency was "seldom the sighting or dominant eye." Schoen and Schofield (25), on the other hand, found no significant difference in diplopia threshold and a significantly greater duration of post direction diplopia in the "dominant eye." In 9 out of 10 subjects, they found greater "neuro-muscular efficiency" in the "non-dominant eye," but the differences were slight.

The greater activity of one eye in retinal rivalry has been suggested as a better test of eyedness than sighting (27). The fact that there is little agreement between the two situations is better explained, however, by the recognition that each measures a different function, rather than a hypothetical general factor of dominance. Similar conclusions are indicated by other ocular measurements, in which, if a consistent laterality is found, there is little agreement with other functions.¹

NEUROLOGICAL EVIDENCE

Underlying most descriptions of ocular dominance is the assumption, either implied or explicit, that ocular dominance is one manifestation of cerebral dominance. However, neurological evidence based on pathological and experimental data does not support this point of view. The distribution of both sensory and motor nerves is evidence against the conclusion that any so-called ocular dominance is an indication of central laterality.

The evidence concerning the motor nerves indicates that some of the fibers cross and others do not (10). The fibers of the third nerve

¹ Witty and Kopel (29) reported that there is no relation between "laterality," including "eyedness," and reading disability. They compared 100 poor readers, all above 80 IQ, with a control group of normal readers. "Handedness" was determined by a questionnaire, "eyedness" by 3 sighting tests. There was no relationship between handedness or eyedness and the degree of reading disability; mixed eye-hand dominance had no bearing on reading. Crosland and Anderson (8) found greater score in "range of attention" tests on the side of the sighting eye. The dominant eye in sighting was determined by 4 sighting tests in which it was reported that there were no inconsistencies among 120 subjects. Other investigators have failed to find such agreement.

are partly direct and partly crossed. The decussation of the fibers of the fourth nerve is probably incomplete also, although there is some evidence that it is complete. The fibers of the sixth nerve do not cross. In other words, the motor functioning of each eye is controlled by both cerebral hemispheres.

The evidence for the optic nerve (sensory) is similar (10). Both cerebral hemispheres are represented in each eye in the peripheral parts of the retina. The left half of each retina is represented in the left hemisphere, and the right halves in the right hemisphere. The neurological data for the macula are somewhat confusing. There is evidence in such phenomena as the "sparing of the macula," after certain cerebral lesions, that the macula receives fibers from both hemispheres, but the evidence is not final on this point. Harris (15), for example, presents an opposing point of view. Fox and German (11), after reviewing several cases, conclude that the macula is not bilaterally represented.

These data, for both sensory and motor functions, indicate that an ocular dominance in any specific situation cannot be considered as a manifestation of dominance of one cerebral hemisphere. *Since the functioning of either eye as a whole is dependent upon both cerebral hemispheres, such functioning cannot be an indication of dominance of either hemisphere.*

CONCLUSIONS AND RECOMMENDATIONS

Since the term "dominant eye" appears frequently in the literature, and since it will probably continue in popular use, there is need for recognition of certain limitations in the use of the term. Eye dominance as a single unitary factor does not exist. Laterality of eye functioning is specifically determined by the situation in which the measurement is made. The relationships between the eyes in such specific measurements is not an indication of any cerebral dominance. Sensory neural organization indicates that the problem of central functioning involves determining relationships of the two halves of the retina rather than the two eyes as a whole.² The motor

² It may be mentioned that several studies have been reported of the laterality of peripheral portions of the retinas. Franz *et al.* (12) studied asymmetry of perception, learning, etc. Gaskill (14) found the 2 halves of the retinas not significantly different in discriminating light intensity. On the other hand, Jasper and Raney (17) have found consistent laterality for the direction of perception of apparent movement in the periphery of vision.

functions of the two eyes are controlled by both hemispheres and cannot be studied from the point of view of cerebral dominance.

In view of these facts, it should be urged that in the use of the term "ocular dominance" the specific method of measurement be included in the statement of laterality. From this point of view, even the unqualified use of the term "sighting eye" is questionable since the situation frequently determines the eye used in sighting.

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ON CITATIONS TO SCIENTIFIC LITERATURE

It seems to me about time for someone to go on the warpath against careless and inadequate citations of professional literature. The undersigned has just been reading a book of which a major claim to recognition is—to quote the publisher's blurb—that it “summarizes more of the literature on . . . than any other book available.” But the method by which one is referred to the sources is faulty in the extreme. In the first place, authors are referred to by their last name without initials, and books are cited without dates or publishers; journals are referred to by the volume number only without the title of the article or the year of publication. This lack of dates is peculiarly trying when the material cited covers a wide chronological range. Here, for example, is one footnote as given by the author. “See, for example, the report by Brainard in *Journal of Genetic Psychology*, Vol. 34, 231–254; Hall, *Child Study Monthly*, Vol. 2, pp. 394–407; Moore, *The Mental Development of a Child*; Shinn, *Notes on the Development of a Child*.” The respective dates of publication of these references are 1927, 1896, 1896, and Vol. I, 1894, Vol. II, 1907. Is not one's judgment of these works materially influenced by their dates? Is it not therefore fair to ask that they be given?

There are several methods of citation which may be regarded as standard. The chief psychological journals have agreed upon one of these methods but there are others which may be equally good. In any case, is there any good reason why an author should not select and with reasonable consistency follow one of these methods? All accepted methods give the author's initial, and give the date of publication.

Something should be done as well against the abuse of *op. cit.* and *ibid.* Let me give an actual example. On page 168 the author implies (what I am sure now he does not actually mean) that a certain person by the name of Gilbert is the only source as to the differential fatiguability of children at various ages. This statement happened to interest me. The footnote merely mentioned “Gilbert, *op. cit.*” I wondered who this Gilbert was, and when he had done his work. A hasty search over the few pages immediately preceding

this footnote disclosed none which had previously referred to him. I was finally compelled to turn to the index of authors' names. There I found the work in question was cited on page 155, that is, just 13 pages away from the place where I found it referred to by means of *op. cit.* Moreover, the reference was to an almost antediluvian study from the Yale laboratory in the 1890's, but the only clue to this was the fact that it was in Vol. 2 of the *Yale Studies*—a clue not entirely clear, I submit, to the average reader of a textbook.

Now let us observe what our author asks of an inquiring reader. He must either accept the authority of one "Gilbert" who is otherwise wholly unidentified; or first hunt through a large number of pages to find a previous reference to Gilbert; or look in the index and then turn to the appropriate page where the appropriate reference to J. A. Gilbert is given. When he does get to the original citation he still will not know what Gilbert was writing about or when—facts of critical importance, particularly when one is assured that this is the only study of the topic in question. And this sort of thing is repeated on page after page, for the book is indeed replete with references to the literature.

I submit that this is an outrage. A man who is doing what is essentially a literary or reporting job on the work of others, owes it to us to report it in the most intelligible possible fashion. When it is made difficult or impossible to identify sources, when we almost have to go to the library ourselves to check references if we are to interpret the book at all critically, then it becomes a grave question whether the book serves a useful purpose.

Now, of course, the book just criticized as a horrible example is only a textbook. That, to my mind, only makes the matter worse. It is only too true that it is a rare student who cares to go beyond what "it says in the book." But we should not discourage such a tendency by making it extremely difficult.

Is it not time that we serve notice on author and publisher that we will not use in our classes any textbook which does not follow standard practice in the citation of the literature and which by copious use of *op. cit.* and *ibid.* makes it difficult to know by whom, where, and when a piece of research was published? Any economy of space gained by omitting essential data is dearly bought.

H. B. ENGLISH.

Ohio State University.

MEMORIES OF THE EARLY DEVELOPMENT OF THE
PSYCHOLOGY OF ADVERTISING SUGGESTED BY
BURTT'S PSYCHOLOGY OF ADVERTISING

BY H. L. HOLLINGWORTH

Columbia University

This article began as a review of Burt's *Psychology of Advertising*.^{*} In reading the volume old embers were fanned into flame. Just a quarter century ago the reviewer's own book, now an antique, on that subject was published, after having first run serially in an advertising magazine. In these twenty-five years the psychology of advertising has developed into a substantial and accepted body of material. No field of applied psychology can point to a more consistent and cumulative growth, in which later steps confirmed and incorporated earlier findings, with a minimum of theoretical disputes.

Many of the incidents of this growth have not been recorded, just as perhaps a good half of the investigations conducted have never been published. The latter is a fact because they were often sponsored by business interests who found the results too valuable to be made immediately available to competitors who had assumed none of the initial expense. The former is true chiefly because few who have more recently written were in the arena when the game began.

Each of the earlier workers in this field could probably narrate interesting incidents of those days when applied psychologists were pariahs whom the anointed would scarcely tolerate in the temple, especially if these latter were not themselves psychologists. It required just these twenty-five years for applied psychologists to form a national order of their own, with their own sacraments and taboos. It would be interesting to collect the memories of these early adventures in business psychology and patch them together into a picture that could otherwise only be guessed at. Some historian should assemble such records before they oblivisce.

Twenty-five years ago there had been exhortations to make practical applications of psychology, and prophecies of what portentous outcomes might result. In education, which was considered a more or less sanctified field, real progress had been made, and in this dis-

^{*}H. E. Burt, *Psychology of Advertising*. Boston: Houghton Mifflin Company, 1938. Pp. x+473.

cussion I wish specifically to exclude educational psychology from any of the comments made. Applications outside the school were tacitly assumed to be unclean. Inquiries and appeals for help from salesmen, employees, manufacturers, lawyers, advertising men, were often either evaded by the seniors or at best referred to younger and more venturesome spirits in the laboratory, who had as yet no sanctity to preserve.

In the years just preceding the quarter century now closing Gale had reported in an inconspicuous way his pioneer experiments with advertisements. Scott had published a volume on advertising, in the title of which, at the publisher's insistence, the word "Theory" was substituted for the word "Psychology." Muensterberg had preached the "psychology of the market place," but the triviality of his experiments gave the wrong chroma to the topic.

In 1910 there was given in University Extension, then supervised by Teachers College in Columbia, what was perhaps the first regular course to be called "Applied Psychology" (outside the field of education) carrying university credit. It was a one semester course, offered by a man who had just received his Ph.D. and had been designated "tutor in psychology" in Barnard College. It was attended by five students and the instructor's "honorarium" was \$75. Among other topics, each of which has since become a book, there were included three meetings devoted to "psychology in advertising."

The Advertising Men's League of New York City had recently organized to put their work on a respectable and professional basis, and they were conducting two lecture courses, on "English in Advertising" (Hotchkiss) and "Art in Advertising" (Parsons). Learning that somebody was to give three lectures on "Psychology in Advertising" at Columbia, they delegated a committee of three to attend and report on this dubious proceeding. The committee endured the lectures and the League requested that they be expanded into a series of ten and given to their members on their own premises. This was agreed to, with stipulations of coöperation and access to files, records, materials and campaign plans.

This series was given many times and to varied groups, at "Round Tables," in hotels, at the Aldine Club, and elsewhere. Interest was lively, and the program of investigation outlined was welcomed after a few preliminary samples, to the extent of raising funds to support a full time graduate research fellowship for the investigation of problems in the psychology of advertising. This fellowship fund was offered to Columbia University. It is worth

recording that the Trustees of this institution refused these research funds, for this is a concrete illustration of the dread of industrial contact prevalent in educational circles at that date.

The idea of "sponsored" or "subsidized" research had not yet stifled individual initiative and made spontaneous effort appear trifling. Industrial provision of research funds and of personal stipends for investigations conducted by men also engaged in academic activity was definitely under suspicion. At this same time the writer, in publishing the report of his caffeine experiments, felt it necessary to introduce the volume with a justifying preface which now sounds abjectly apologetic. Nevertheless, at a subsequent meeting of the American Philosophical Association a Johns Hopkins professor whispered in scandalized breath to a philosopher from Columbia—"Did you know that Hollingworth received funds for his caffeine experiments?!" Perhaps these early insinuations die hard. At any rate, in the quarter century since that time Judas has never been able to secure research subsidies from any source except the hard-headed business man.

Subsequently the Columbia trustees' blockade, wherever it was located, relented. The fellowship fund was accepted by the University and in similar overtures since that time there has always been cordial coöperation in ethically conceived plans for the business endowment of research. E. K. Strong, then or recently assistant in the Barnard laboratory, was appointed to the fellowship. His "Relative Merits of Advertisements" was a startling title in the sober *Archives of Psychology*. This fellowship was continued by larger organizations of advertising men with whom the pioneer League merged, and Strong under these auspices carried through extensive researches, which are well known to later workers in this field.

My own *Advertising and Selling*, embodying the course of lectures referred to, was published in 1913 by Appletons for the Advertising Men's League, which shared in the royalties so long as there were any. Its chapters had, during the previous year, appeared serially in "Judicious Advertising," a magazine for the trade, published by Lord and Thomas of Chicago.

In time the League's organized educational program, including a later course on "Economics in Advertising" (Tipper) was taken over bodily by the School of Business of New York University, and this was one of the steps in the development of its vigorous Division of Marketing. The "four horsemen," as they were then called (Tipper, Hotchkiss, Hollingworth and Parsons), were appointed

lecturers and collaborated in the production of *Advertising, Its Principles and Practice* (1915), which attempted to put in a single volume all the aspects of advertising except the office details.

Local developments proceeded at such a pace that it would be tedious to chronicle them. My own work in this field was shortly limited to the extension courses in Columbia, where a course in vocational psychology was also developed, along with a more general course in applied psychology. Poffenberger entered the field and soon assumed entire responsibility for these developments; the work in advertising was then taken over by Nixon, with a more elaborate program in the School of Business. Franken carried on the psychology of advertising at New York University, and also took over certain consulting connections which I had engaged in up to the time of the War. The fellowship ceased and Strong's work was continued elsewhere.

Similar developments were in progress in other centers, and memories of these could profitably be recorded by those in touch with them or responsible for them. Scott's second book dared to use the word "Psychology" in the title. Starch, Adams, Kitson, Burt, Poffenberger, and more recently a host of active and prolific younger men too numerous to list could all contribute memories of early adventures in the rather remarkable story of the development of a well-knit and substantially scientific chapter in the history of psychology. Some of these memories might antedate the events here sketched. A rather surprising group of psychologists have somewhere published a single paper in this field. The circumstances of this isolated act might be interesting. A few examples from memory are Brown, Dorcus, Knight, Laslett, Langfeld, Newhall, Thorndike, Yerkes, Warden. The submerged reports of investigations "not for publication" (there are at least 40 in my own files) would provide a mine of material if they could be excavated.

Actual developments took what was for some an unexpected and even an unwarranted turn. There seemed to be a feeling among the old masters, even those whose tolerance overlooked the practical character of the activities, that the youngsters were overstepping the bounds of propriety in presuming to instruct and correct advertising men in their practices. The writer, for one, was warned that psychologists could more profitably *use* the advertiser's materials as *illustrations* of psychological laws. "One could show how Weber's Law and the Curve of Forgetting are demonstrated in these practical things."

But for these youngsters the mere finding of lively examples

wherewith to illustrate armchair lectures was trifling with an opportunity. The viewpoint and technique of the laboratory were carried over bodily into the print shop, the copy room, the studio, the factory, the consumer study, the sales and marketing program. Trade-marks, slogans, packages, headlines, copy, cuts, letter series, magazine dummies and car cards were dragged into the laboratory to replace the sacred lifted weights, series of grays, and nonsense syllables. The psychologist became an expert adviser rather than a mere camp follower and often found enough extracurricular activity and consultation to take the place of more scholarly diversions such as billiards and chess. Whatever the impropriety of such behavior, that is what gave the psychology of advertising a substantial foundation. Its observations stand and cumulatively grow while structuralism, organicism, anthroponomy and topology in turn flare up and expire.

The Economic Psychology Association was formed in these days, for the support of research in applied psychology by industry. In this organization the writer was psychological instigator, as well as being responsible for its dissolution. Experience in that connection induced an initial sceptical attitude toward the destiny of the Psychological Corporation, established years later with the same aims, but by an earlier scientific generation. The old guard have probably never quite understood the lukewarm enthusiasm of younger and actually active applied psychologists for paper organizations that do not embody the consecrated energy of some individual.

This brings us, finally, to the ostensible excuse for this preamble—the publication, at the end of this quarter century, of one more organized survey of the accomplishments in a special field, Burt's volume on *Psychology of Advertising*.

The volume gives a well organized review of the material in this field, which has now become fairly well standardized, and also has certain distinguishing characteristics of its own. The topics are illustrated by a wealth of concrete and effectively chosen examples from current observation, as well as elaborated in the light of experimental findings.

There are twenty-four chapters and an appendix. Most of the established captions occur—suggestion, attention, memory, long and short circuit, mechanical and interest devices, position, color, size, intensity, motion, contrast isolation, elements of design and arrangement, the novel, the comic, typography, relative strength of appeals, trade-marks, pictures, individual differences. There are also chapters on methods, the consumer's wants, adapting appeal to prospect. There is a chapter on radio as an advertising medium, presenting

material on many of the new problems that arise in that connection, one on "Other Advertising Media" and one on "Good Will." The appendix gives a description of the technique of determining the reliability of a difference between two measures or averages.

Considerable emphasis is laid on the good will aspect of business, and on the study of the consumer's needs rather than the arbitrary forcing of a commodity. There is relatively little elaboration of measurement beforehand of appeals or campaigns, although attention is called to this practice, and to the use of juries. More attention is given to such experimental work as suggests general principles.

The book contains no pictures of any kind, except graphs. No actual advertisements are presented in the text. Although written with class use in mind, there are no "exercises," no organized bibliography, no index of names. Perhaps the time has now come when this subject need no longer be taught on a laboratory or exercise basis. However, most teachers will wish to make some use of this method, and a set of such materials to accompany this text would be useful, and would perhaps serve better if not incorporated in the book but separately issued.

Each chapter is followed by a summary which comprehensively reviews the preceding discussion. The "historical method" is given a prominence which may overrate its value as an indicator of value, however interesting the historical trends may be in themselves, but this may be a matter for debate.

There are very few corrections to be made in the text as it stands. Typographical errors are few (the reviewer noticed only two). The accuracy of references could be improved, and many users of such a text would be glad to have accessible a larger set of bibliographic materials. For example, the article referred to in the first footnote on page 150 contains no data whatever on the topic in connection with which it is cited.

Intelligence quotients are written sometimes with and sometimes without the decimal. Prunes, that stable item of faculty club menus, are said to be consumed by people predominantly of average or below average intelligence. Maybe so.

The volume is readable, the results of experiments are compactly summarized, without undue technicalities. The book should be read with interest and understanding by the general student and by the man of affairs with no psychological background. It is an admirable running account of a development that psychologists and advertising men of twenty-five years ago never dreamed of.

BOOK REVIEWS

MURCHISON, CARL (Editor), *A History of Psychology in Autobiography*. (Vol. III.) Worcester: Clark University Press, 1936. Pp. xvii+327.

From the point of view of the selfish interests of the reviewer *A History of Psychology in Autobiography* has two remarkable virtues: (1) He can say without reservation that it is a book worth reading and worth owning. (2) He does not have to search for passages which will allow him to prove that he really knows more about the subject than the authors. The authors may not tell the truth, certainly not the whole truth and probably they tell some things that are not quite true about themselves, but the reviewer is in no position to correct them.

If psychology can be described in terms of what psychologists do, such a book as this permits us to see what psychology is by revealing what they do. To use terms from the vocabulary of the college sophomore, it shows that they introspect, that they rationalize, and that they alibi. Perhaps that is what psychology is, a conglomerate of projections of personal hunches, of explainings away of unpleasant inadequacies, and of excuses for not doing what we set out to do.

Obviously, the present authors do not employ these ingredients in the same proportions. The very nature of autobiography as a medium compels some introspection, but with Bartlett it is at a minimum. He shows himself doing his job and telling what he conceives that job to be, a job with many aspects, not to be shirked because of systematic bias, and a job that one likes. On the other hand, Froebes, best known to us through his scholarly textbook, reveals that his whole existence is spun out of the gossamer exuded from the processes of his mind. His books are not only his life but his world.

Among the rationalizers, Otto Klemm easily leads. This is surprising because one would suppose that his long concern with practical applications of psychology might be associated with a taste for actuality. Instead his life sounds like that of a mystic; nothing has happened naturally to him; nature and circumstances conspired to guide his steps. At the other extreme stands C. S. Myers with an

orderly account of a life of distinguished achievements, including the winning of academic status for psychology at Cambridge and the development of the National Institute of Industrial Psychology.

One should not look for alibis from those in lowly positions. This book is full of them, but James R. Angell, perhaps because he has held such eminent positions, provides the largest quota. In this paper, by far the best in the volume, he gives evidence of what a great psychologist can do, evidence of what he could have done, and reasons why he fell short of accomplishment. There is a note of genuine tragedy in the enumeration at the end of the paper of a list of his students. One is forced to infer that a great teacher looking back over fifteen years during which he might have exerted a determining influence in the history of psychology for the next generation must be thinking that the list of his students stops too near its beginning. John B. Watson also has much to explain, but one has the feeling that he did not abandon psychology willingly. He himself notes that Angell once reproached him for his unacademic activities. But to the reviewer it appears that Watson's desertion of academic psychology is better explained than is Angell's.

Not all of these psychologists have had to make decisions which they feel obliged to explain. One has the feeling that Thorndike, Bartlett, and Myers are telling straight stories; certainly Carr is, and possibly Wirth. Wirth, however, like Klemm and Froebes, confuses actual events with thoughts, dreams, and theories.

The contribution which this book makes to the history of psychology, and after all such a contribution is its professed reason for being, is very uneven. In general there are inverse relations between the human and historical values. Madison Bentley speaks from behind a screen, showing nothing of himself, but describing what he sees in the passing scene. At the other extreme Sante de Sanctis paints himself, his family, and his career in Gargantuan proportions but tells us nothing of any historical interest. Somewhere between these extremes Scripture finds it possible to criticize the whole development of modern psychology in terms of his own predilections. Of all the contributors only Angell and Marbe seem able to see their lives and their contributions to psychology as integrated wholes. Angell's article describes the origins and rise of functional psychology clearly, authoritatively, and in historical perspective. It is a document which must be studied by anyone who attempts hereafter to write a history of psychology in America. Watson, on the other hand, fails to give any intelligible account of the rise of behaviorism.

The future historian will look in vain in this autobiography of the dominant figure in America's most permeating psychological movement for any clue as to its intellectual origins. Its emotional origins are clear enough. Watson was a rebel, and behaviorism was a rebellion, but why did the rebellion assume this particular form? Did Watson get any ideas from Pavlov or Beckterev? When did he read any of their works? He refers us to Dunlap, mentions Yerkes' work at Hopkins on conditioning, and expresses indebtedness to Lashley and to several others, but he makes no attempt to connect these fragments in his own life. The time of emergence of his ideas he does tell us, but not what were the points of departure. On the other hand he does not assert, in so many words, that he invented behaviorism. One infers that it must be so.

Marbe contributes a valuable chapter on the history of the Würzburg school, and lays an impressive claim to the origination of the word-association technique. On the whole his life-story gives us a fairly complete picture of what a psychologist with the old traditions was doing in Germany from 1896 to 1931. Completeness does not imply clarity. It is not possible to make out any clear development of German psychology in the first third of the twentieth century from the obscure accounts of Marbe, Klemm, or Wirth. Obsessed with the importance of men's names and individual tenets; absorbed on the one hand in details of technique and minor experiments, and on the other hand in grandiose abstract concepts, they have passed beyond the security of the structuralism of their youth, through the technicalities of applied psychology out into some vague limbo hardly recognizable as psychology.

The same period in British psychology is adequately portrayed by Myers and by Bartlett. It has been eclectic, pragmatic, free from theoretical qualms, and now inclines toward the practical.

Can we discern from the samples before us any trends in American psychology? Three important movements are represented. Both behaviorism and functionalism are discussed by their best known sponsors in the past tense. Carr, who followed in the functionalist tradition in spite of an adequate exposure to behaviorism speaks like a spectator rather than an active participant in any definite movement. The impression remains that functionalism has come to rest; but not that it has achieved a goal.

The historians of a third great American movement in psychology will find little documentary material in Thorndike's account of his life. By reading between the lines one may infer that Thorndike

believes that hard work and an open mind are more important than system and theory. One step farther might lead one to hope that such a pragmatic attitude will ultimately bring some order into the chaos of the present psychological scene. Unless such a step can be taken the present volume is closed with the disheartening impression that the psychologists who have contributed to it are confused, apologetic, and uncertain of their objectives.

WARNER BROWN.

University of California, Berkeley.

LUCKIESH, MATTHEW, and MOSS, FRANK K., *The Science of Seeing*. New York: D. Van Nostrand Co., 1937. Pp. x+548.

The authors, who are well known for their contributions to the literature of light and illumination, offer here a new approach in the same field, which they intend to define in the title they have chosen. In the first two chapters in particular and in some measure throughout the book, the authors demonstrate the great practical importance of "seeing," but their arguments for its status as a science are not convincing. The dominating motive of utility and the ever-present topic of illumination as well as the wide distribution of the sources from which material has been drawn leads us to characterize the work as technological. There is no intention of "hair-splitting" on this point. The reviewer feels that a clear understanding of what the particular mode of attack implies will prevent disappointment for a reader who fails to find what he expects, and consequent unevaluating criticism. A scientific treatment of so broad a field must inevitably contain innumerable lacunae, will indicate many unsolved problems, and will, therefore, leave an impression of incompleteness. The authors of *The Science of Seeing*, because of the wealth of material cited and because of the endeavor to make that material solve the practical problems as adequately as possible, present a discussion that is apparently complete. Such problems as remain will shortly be encompassed by the illuminating engineer. Nevertheless, there are many unsolved questions. To cite but one example, the desirability of the use of various colored inks in printing on different colored papers is settled, perhaps correctly for practical purposes, on the basis of relative reflectances. No reference is made to any experimental literature, the implication being that there is none. It is true an appeal is made to "psychological factors" of an aesthetic nature, but as usual the psychological aspects become somewhat mystical.

The authors cannot be taken too severely to task for such an attitude because much more is known popularly about statistical methods in psychology than about the laboratory technique of psychological observation. Perhaps this tendency would not appear so important to another reviewer.

On the positive side, the assembling of the technical data on illumination and its readily comprehensible presentation will prove of very great value to persons who have occasion to acquaint themselves with that material and to make use of it. Numerous tables, graphs, and schematic diagrams present the facts from all angles. A large proportion of these are physical in nature, but a thorough-going attempt has been made to relate them to the best available physiological theory of the photo-reactive human organism. It is toward a combination of all factors for solution of such problems as "Reading as a Task" and "Eyesight and Seeing" that the book is directed.

A bibliography of 184 titles, many of them by one or the other of the authors, is appended, together with an adequate index. Specialists will still need to go to the original articles, but for a thorough-going summary and interpretation of the many subjects treated, and an indication of the sources of the material, many workers will find *The Science of Seeing* valuable.

FORREST LEE DIMMICK.

Hobart College.

GARDINER, H. M., METCALF, RUTH CLARK, and BEEBE-CENTER, JOHN G., *Feeling and Emotion: A History of Theories*. New York: American Book Company, 1937. Pp. xiii+445.

This is a comprehensive survey of the theories of feeling and emotion, beginning with Heraclitus and continuing down to the present. The first nine chapters are largely the work of the late Professor H. M. Gardiner, professor of philosophy at Smith College. Upon his death, Professor Metcalf revised and completed this section. The final two chapters on nineteenth and twentieth century theories were contributed by Professor Beebe-Center.

It is very definitely a book for the specialist. The closely reasoned, tightly written pages are not to be taken too easily or rapidly. Only rarely, however, is the condensation carried to the extreme where the text becomes a mere catalogue of names and viewpoints. Considering the difficulty of Professor Gardiner's task, he has done remarkably

well. He can write critically, but always with balance and tolerance. There is an intangible air of quiet, scholarly good nature which goes far to lighten the heaviness of the subject matter. When he writes of ancient theories, he does it sympathetically, but without showing the common trait of scholars to deify the historical. The older contributions are ably presented in the intellectual context of their period, at the same time being tolerantly evaluated in the light of contemporary knowledge. The result is a book which is not always easy reading, but is never painful.

Nor have his collaborators let him down. Professor Metcalf's contribution must have been a large one, as the evenness of the book testifies; and Professor Beebe-Center has given us an excellent survey of the modern theoretical contributions in his two chapters. The selection of contemporary material which has not as yet been placed in historical perspective is always hard. Beebe-Center has chosen cautiously and wisely. One might wish that in dealing with Head's thalamic theory he had included some of the critical comment of Kinnier Wilson and others on the opposite side. One might have hoped for the inclusion of Carr's judgmental theory of pleasantness and unpleasantness, particularly in view of its increasing influence in the field. But these are minor matters in view of the general excellence of the chapters. A survey of this sort cannot contain everything being done today. It is refreshing to find included a summary of the stimulating work of Dumas, too often neglected by American psychologists.

The book contains a comprehensive bibliography and a very complete index. This last is invaluable in a treatise of this sort.

WILLIAM A. HUNT.

Neuro-Psychiatric Institute of the Hartford Retreat.

LINDQUIST, E. F., *A First Course in Statistics: Their Use and Interpretation in Education and Psychology*. Boston: Houghton Mifflin Company, 1938. Pp. xiii+226.

LINDQUIST, E. F., *Study Manual for a First Course in Statistics*. Boston: Houghton Mifflin Company, 1938. Pp. 122.

The avowed purpose of the author is to stress the critical use and interpretation of a restricted number of statistical techniques rather than the mathematical aspects and the mechanics of computation. The study manual, which parallels the text, is therefore different from the usual manual in that instead of consisting largely of com-

putational exercises it attempts through questions and problems to help the student find out for himself the meaning and limitations of the various techniques. Consequently the text, when considered independently of the student's answers to the questions in the study manual, is not a good source of information concerning statistical methods. On the whole, the questions in the manual are excellent and no doubt thought provoking, but a few seem trivial and occasionally the reviewer found himself wondering how well statisticians would agree in answering certain questions.

The material included is restricted to what would ordinarily be covered in a semester course. Consideration is given to the following topics: graphic and numerical description of frequency distributions, the normal curve, sampling, comparable scores, product-moment correlation, and the reliability and validity of tests. Aside from the last chapter, the author has nearly succeeded in discussing statistics in words of one syllable, and in general this over-simplification, while at times a bit tedious, is not accompanied by a loss of rigor.

When one considers the aim of the author, it is surprising to find so much fuss over the mechanics of interval limits and midpoints. Few statisticians will agree that having the midpoint a multiple of the interval size leads to a computational advantage which outweighs the tabulating convenience that usually results when the expressed lower limit is a multiple of the size of the interval. The discussion of sampling error theory, which is superior to most elementary treatments, could be improved by first giving an exposition of the rudiments of probability theory. The concept of correlation is developed via the fact that r is the average of the product of paired standard scores, and the student is advised to "do all of his thinking about the correlation coefficient in terms of the relatively simple z -score definition" (p. 159). Fortunately, the text goes beyond this to discuss the meaning of r in terms of regression and the error of estimate.

This text would be disappointing to a reviewer who finds delight in pointing out errors. There are, however, a few questionable concepts. For instance, the statement "a difference is either significant or not significant statistically—we should not speak of degrees of significance" (p. 123) cannot be defended. One erroneous notion, which Professor Lindquist seems to share along with a large number of psychologists and educators, is that a meaningful answer can be given to the question "what is the probability that the true mean

of a population falls above or below a certain point?" If one examines critically the statement that "the chances are 78 in 100 that the true mean lies between 76 and 78 pounds" in the light of the fundamental definition of probability in terms of events, it will be found that there is no empiric, theoretic, or imaginative way of enumerating events. How can we say how many events are favorable to the occurrence of a true mean within defined limits when we cannot conceive of a frequency distribution of true means?

Although Lindquist's *A First Course in Statistics* is fairly sound from a statistical viewpoint, the reviewer hesitates to recommend it for two reasons: first, because he feels that the text is too incomplete or restricted in material, and second, because he wonders about the feasibility of the pedagogy involved in teaching elementary statistics by the so-called Socratic method.

QUINN MCNEMAR.

Fordham University.

MAY, MARK A., and DOOB, LEONARD W., *Competition and Coöperation*. New York: Social Science Research Council, 1937. Bulletin No. 25. Pp. 191.

ALLPORT, GORDON, MURPHY, GARDNER, MAY, MARK A. (Chairman), and Research Assistants on Sub-Committee on Competition and Coöperation, *Memorandum on Research in Competition and Coöperation*. (Mimeographed.) New York: Social Science Research Council, 1937. Pp. 389.

In 1934 the Social Science Research Council found itself confronted with the quandary of an expanding program of research projects proposed by its Committee on Personality and Culture, and an inadequate budget. It was decided to meet the difficulty by limiting the immediate program to the appointment of three sub-committees, charged to make surveys of certain research areas, namely, Competition-Coöperation, Criminal Causation, and Acculturation. Each survey was to undertake a critical analysis of the state of present knowledge, outline existing frontiers, and list feasible research problems for filling in the gaps and advancing the frontiers. The two publications here reviewed present the report of the first-named of these sub-committees.

Members of the committee were aided by eight research assistants, each charged with surveying, abstracting, and analyzing the research

literature in certain portions of this diffuse and ill-defined area. The six appendices in the *Memorandum* present these abstracts and reports, somewhat abridged, on researches on coöperation-competition in children (B. Burks), in adults, mostly statistical-experimental studies (L. Doob), in life-history documents (J. Dollard), in primitive cultures (M. Mead), in sociological studies of several types of group (J. H. Unseem, C. Q. Berger), in economic coöperative societies (D. W. Oberdorfer), and in the extensive Russian literature (J. W. Boldyreff). The literature abstracted includes in all 965 titles; and while of course this is not a complete survey of the field, it is designed to be both diversified and representative. This *Memorandum* serves as the basis for the May and Doob monograph. Along with these two volumes should be mentioned two others which developed out of this inquiry, but were published independently of it, Dollard's *Criteria for the Life History* (New Haven: Yale University Press, 1935), and Mead's *Coöperation and Competition Among Primitive Peoples* (New York: McGraw-Hill Book Co., 1937).

The common notion that competition and coöperation are antithetical is erroneous. As the authors point out, "the opposite of 'competitive' is not always 'coöperative,' but may be 'uncompetitive': and 'uncoöperative' may be the contrast to 'coöperative.'" Both are learned forms of behavior, directed toward the same social end, the satisfaction of individual wants in a social situation. Much behavior is at the same time, in different aspects, both competitive and coöperative. In competition, the end sought can be achieved by some and not by all the individuals thus behaving; whereas in coöperation it can be achieved by all or almost all concerned. It is thus the scarcity or abundance of that which individuals seek (material objects or prestige) that determines primarily which form their behavior will take, and not a postulated innate drive toward competition or coöperation.

Efforts to find answers to the four crucial questions, "Why, For what things, With what persons, and In what manner, do individuals compete or coöperate?" have led to enormously varied methodological attacks. To synthesize this chaos, the authors offer a theory of competition-coöperation which not only offers a conceptual framework into which varied findings may be fitted, but, they believe, also has value as a basis of prediction, as an index of the significant and crucial research problems, and as a guide to improvement of methodology. The theory is expressed in a set of eight postulates, which,

because they are central to the whole scheme, are here reproduced in full:

Postulate 1. The social form of the behavior of an individual or individuals in a situation is a function of and is defined by the goals, persons, rules, and performance that are inferred to be operating.

Postulate 2. The psychological form of the behavior of an individual or individuals in a situation is a function of and is defined by the discrepancy (between "level of achievement" and "level of aspiration"), knowledge, attitude, and skill that are inferred to be operating.

Postulate 3. The social form of the behavior of an individual or individuals in a situation is a function of the psychological form of the behavior; or, the goals, rules, persons, and performances of the individual or individuals in a situation are a function of his or their discrepancies, knowledge, attitudes, and skills.

Postulate 4. The goals, rules, persons, and performance in a given situation are functions of the history of the culture; the discrepancies, knowledge, attitudes, and skills of an individual are functions of his life history.

Postulate 5. On a social level, individuals compete with one another when: (1) they are striving to achieve the same goal that is scarce; (2) they are prevented by the rules of the situation from achieving this goal in equal amounts; (3) they perform better when the goal can be achieved in unequal amounts; and (4) they have relatively few psychologically affiliative contacts with one another.

Postulate 6. On a social level, individuals coöperate with one another when: (1) they are striving to achieve the same or complementary goals that can be shared; (2) they are required by the rules of the situation to achieve this goal in nearly equal amounts; (3) they perform better when the goal can be achieved in equal amounts; and (4) they have relatively many psychologically affiliative contacts with one another.

Postulate 7. On a psychological level, an individual competes with others when: (1) there is discrepancy between his level of achievement and his level of aspiration; (2) his knowledge of the goal that he seeks indicates that it is limited and cannot be shared at least equally by other persons in that situation; (3) his attitudes produce within him a state in which his attitude toward competing overbalances possible conflicting attitudes toward potential competitors, toward the rules of the situation, toward coöperating rather than competing, etc.; and (4) his skill is of such a nature that under the rules of the situation he has a reasonable chance of success by competing.

Postulate 8. On a psychological level, an individual coöperates with others when: (1) there is a discrepancy between his level of achievement and his level of aspiration; (2) his knowledge of the goal he seeks indicates that it can be reached by striving with others; (3) his attitudes produce within him a state in which his attitude toward coöperating overbalances possible conflicting attitudes toward potential coöperators, toward the rules of the situation, toward competing rather than coöperating, etc.; and (4) his skill is of such a nature that under the rules of the situation he has a reasonable chance of success by coöperating.

Into this framework the authors then proceed to set representative samplings of the multitude of researches, problems and findings. Conclusions from many experimental (chiefly "psychological") studies of competition-coöperation are summarized in 9 propositions, very general and formal, but relevant to the 8 basic central concepts—goal, rules, persons, performances; discrepancy, knowledge, attitude, skill. The sociological studies seem to fit less satisfactorily into neat categories; but the main findings are summarized in terms of postulates 7 and 8, since (according to postulate 3) the psychological conditions of competitive-coöperative behavior must be taken as fundamental. Competition and coöperation, as they appear in a half-dozen types of group organization, are reviewed and a series of generalizations formulated in terms of the basic theory. Studies of competition-coöperation as they function diversely in broad anthropological culture patterns lead to 9 more general propositions with respect to determining conditions. Of particular interest, of course, are those brought out by the Russian experiment. Finally, the life-history (*i.e.* clinical) approach, which contributes a half-dozen more propositions to the summary, seems to be regarded by the authors as perhaps the most searching and comprehensive of all four methods of revealing and evaluating the factors in competitive-coöperative behavior, even though its limitations and defects are critically recognized and stated.

In the concluding chapter, on "Prediction and Future Research," a list of 68 problems is offered for future investigators to answer. Phrased as these are, in terms of the authors' synthesis, their relations to one another are clarified, even though their variety is great. To this extent, then, the authors' purposes may be said to have been achieved; although it is quite inevitable—probably desirable—that in so brief (and hence abstract) a summary, one finds many more questions raised than answered.

On the whole, this group of documents offers one answer, and apparently a worth-while answer, to the question of how coöperative research can be effectively conducted. In terms of the theory itself, social research need not be compelled to choose between being competitive and coöperative; there are many ways in which it can be both. At least, a thought-provoking program has been suggested.

F. A. KINGSBURY.

The University of Chicago.

BIERENS DE HAAN, J. A., *Labyrinth und Umwege. Ein Kapitel aus der Tierpsychologie*. Leiden, Holland: E. J. Brill, 1937. Pp. 231.

The author's purpose in writing this monograph, as stated in the preface, was threefold: (1) To introduce the reader, by means of an historical-critical survey, to the problems and methods employed in the maze and the detour (Umwege) experiments; (2) to present a summary of the work done with both techniques on the various animal species; and (3) to attempt to combine the results obtained from use of these two techniques and to find a common explanation of the behavior of the animals in terms of the grasping (primary or secondary) of spatial relations in reference to the goal striven for by the animals.

In the first three chapters of the monograph the author deals with an historical and critical account of the maze and detour experiments. Beginning with the maze technique, he gives a rather complete description of the various types of maze, followed by a critical consideration of the various measures of learning and of the reliability of the maze. Thus far the monograph deals entirely with the work of American students and consists of the usual treatment of the literature. Nothing appears in this treatment which is new for American students but, as the author states, it was written especially to introduce the maze and its problems to his European readers. From this treatment of the maze he turns to a consideration of the detour experiment and on the basis of the early experiments by Hobhouse and subsequent experiments by Köhler and others he distinguishes three types of detours of increasing difficulty for the animal: (1) simple detours in which both the goal and the way to the goal can be perceived by the animal at the starting point and in which it is not necessary for the animal to turn its back on the goal in traversing the detour; (2) detours in which both the goal and the way to the goal are visible to the animal from the starting point, but in order to make the detour the animal must temporarily lose sight of the goal; (3) detours in which the goal is visible at the start, but the detour is, at least partially, invisible and consequently must be known more or less from previous experience; and in which the goal is, at least temporarily, invisible while the animal is making the detour. On the basis of his earlier examination of the maze the author now describes the maze experiment as a detour of a fourth degree of difficulty for the animal, viz., a detour in which, at the start, both the goal and the more or less intricate route to the goal are invisible

to the animal so that the animal must not only know the route from experience but must also know from experience that the goal is to be found in the apparatus.

The following six chapters are devoted to a survey of the work done with the two techniques on various phyla and species of the animal kingdom. In this survey the author proceeds from the clear-cut detour experiments on mammals and intermediate forms to experiments on fish and invertebrates which may be considered as either detour or maze experiments depending upon which aspect of the situation and of the resulting behavior is emphasized. From these simple detour experiments he continues with a survey of the maze experiments proceeding from experiments with the lower invertebrates to those with the higher vertebrates exclusive of the rat.

Approximately one-half of the monograph is included in a single chapter dealing with the rat in the maze. The discussion is concerned primarily with two problems: (1) what is the sensory basis for learning the maze path and for running it after it has been learned; and (2) how is the learning of the right path in the maze effected. Examination of the existing literature on the first of these problems leads the author to the conclusion that in learning the maze the rat may make use of any available stimuli either from within the maze, or from the environment surrounding the maze, or from within its own body. Furthermore, after the maze has been learned the running is not a simple kinaesthetic habit, but is based on an acquired knowledge of parts of the maze and their relative positions and on an idea of the right path. The rat in the maze never becomes a mere automaton, but always continues to be a subject that perceives and remembers and strives to reach a goal. Consideration of the second problem leads the author to accept in a modified form Thorndike's theory of learning in which the working of the "effect" is conceived not in a physiological sense but rather in the manner of McDougall's *hormic* psychology. Basic to the animal's finding its way in the maze are the striving to reach the goal and the feelings of satisfaction and dissatisfaction aroused by success and failure. At the same time it builds up more or less clear ideas of the path leading to the goal and of the relative position of particular points in the maze in reference to the goal. Experimental evidence for his position is presented and conflicting theories, *e.g.* Hull's Goal Gradient theory, are rejected.

In both the detour and the maze technique the author finds the animal placed in a situation in which it is impossible for the animal to reach the goal by going directly to it as is the case when no obstacle

is interposed in its path. In such a situation, provided the problem is not too difficult, the animal, (1) relying on general experience of the spatial relations existing in its environment, acquired during its life time, and utilizing its capacity to apply such experience in a new situation, immediately grasps what path it has to follow to reach the goal; or (2) without such general experience available, must use trial and error behavior until finally the goal is reached. This type of behavior must continue until the existing spatial relations are learned. The differences between these two possibilities are only relative. In both cases the animal strives to reach a goal, tries to do this by the shortest detour, makes use of various sensory cues, forms ideas about the way to the goal, etc. Detour and maze are different only in degree; running the maze can be regarded as the making of a complicated detour. Both are based on the grasping, directly present or gradually developed, of spatial relations with reference to the goal. The maze is a complex detour; running the maze is an integrated making of detours.

It appears to the reviewer that, for the American reader, the chief contribution made by this monograph is found in the survey of the literature on the maze (chiefly an American technique) and especially on the detour (primarily a European technique). This survey is well done and covers the two fields thoroughly, giving ample evidence of the author's comprehensive knowledge of the literature of both fields of research. The appended bibliography contains 250 titles which are covered in the monograph. To aid the English-speaking reader, an English summary of the monograph is also appended.

W. G. McALLISTER.

University of Illinois.

PORTO, CESAR, *L'instinct*. Paris: José Corti, 1936. Pp. 285.

This volume is essentially a metaphysical analysis of the concept of instinct. An attempt is made to show that the basis for instinctivity is not sensation but a strange receptivity having perhaps a magnetic-electric origin, and which is suprasensible and hyperphysical. In this connection the following statement is made: "It is necessary to pay particular attention to the suprasensible impressions which merit the name cosmic, to all those which result from the rotation of the zodiac. . . ." The foregoing excerpt is representative of the tone and content of the whole book. However, it is conceivable

that this most speculative treatise on instinct which leans heavily upon the occult and appeals to it for support may have a proper place in psychological literature, particularly when the study of extrasensory perception is exciting so much attention.

In Part I of the book, entitled "The Metaphysic Life" the author gives a foundation for variability of human character together with an account of "psychic individuality"; from that he leads into a discussion of sensation, suprasensation and the occult in general. The second part of the volume deals more directly with the concept of instinct, first relating it to sensation and suprasensation and following with a discussion of the collective instinct and its place in our social life. Finally, a classification of instincts is given.

T. A. JACKSON.

Columbia University.

DIMOCK, HEDLEY S., *Rediscovering the Adolescent: A Study of Personality Development in Adolescent Boys*. New York: Association Press, 1937. Pp. xx+287.

This book, intended principally for individuals "who are working with or teaching about the adolescent," presents in non-scientific fashion a description of "the adolescent boy and his development from twelve to sixteen." The material for the book was gained from the author's studies of two hundred boys who were between twelve and fourteen years of age at the beginning of the investigation. The studies extended over a two-year period; so the descriptions of the adolescent boy are only partly based on longitudinal data on the same subjects.

The scope of the book is broad, including the fields of play, group formation, friendships, personality, emancipation from parents, moral and religious thinking, age of reaching puberty, and physical growth. The important aspect of heterosexual relationships is not treated; Dimock recognizes this lack. The multiplicity of variables studied regrettably prevented anything like an intensive investigation of a given variable or related variables.

Interviews, observations, questionnaires, performance tests for strength and motor ability, and mensuration for height and weight comprised the techniques employed in the investigation. Their adequacy is not always demonstrated; some of them are admittedly exploratory and crude.

The quantitative data are presented chiefly in terms of averages for which no variability scores are given. The findings of the study suggest that the changes which take place in adolescence are not so numerous and abrupt as have previously been postulated. Among the some twenty-five variables studied, Dimock found only one, physical growth, in which changes during adolescence were really marked. He concludes, "The idea of periodicity in growth should be supplanted by that of continuity." This conclusion should enlighten those theorists on adolescence who look upon the teen age as being a sort of pupa stage in the development of a human being.

The book is valuable in that it presents a straightforward excursion into a perplexing area of human phenomena, an area where few facts exist. It emphasizes above all the need for more experimental work against which the old concepts of adolescent growth and development can be reexamined.

JOHN B. WOLFE.

University of Mississippi.

SLETT, R. F., *Construction of Personality Scales by the Criterion of Internal Consistency*. Hanover, N. H.: Sociological Press, 1937. Pp. vii+92.

In this monograph the author has proposed a series of important questions to workers constructing personality, attitude, and interest scales. Among the many problems posed concerning the construction of scales, perhaps the most important are: can one assume

- (A) that results based on a sample of 100-200 individuals will be reliable?
- (B) that an item cannot discriminate successfully in two or more scales unless the total scores of these scales are highly correlated?
- (C) that items ranked for discriminative value remain highly constant for groups of dissimilar composition?
- (D) that items yielding statistically significant differences between extreme segments, say upper and lower quarters, of the total score distribution measure the same factor?

The answers to these questions were sought by the empirical method and, for the most part, the results were unequivocal and must be considered by future workers.

Six personality scales each containing twenty-two items purporting to measure Morale, Inferiority, Family Adjustment, Economic

Conservatism, Attitude toward Law, and Attitude toward Education, were administered to four dissimilar groups consisting respectively of high school, college, employed, and unemployed individuals. There were two hundred individuals in each group, with the sexes equally represented, making a total of eight groups to be studied for each scale. An attempt was made to control age, marital status, and socioeconomic status in so far as possible.

Four measures of item discrimination are reported, but all are closely related. The first of these is the difference between the mean score on the item for the individuals in the upper and lower quartiles of the distribution. The second measure is this difference divided by the standard deviation of the difference, *i.e.*, the critical ratio. The third index, called the *scale value difference ratio*, is a very ingenious concept. It is a measure of the extent to which factors that are most potent in determining the total score on a set of items are identical to those determining the responses to the item. The last index is the same as the third except that in the computation the item under consideration is eliminated from the total score.

The evidence clearly demonstrates that for groups of 100, values determined by the various indexes are very unstable and that an N of 400 is necessary to yield stable values. Sletto concludes "that what measure of item discriminative value will yield the most constancy in ranking items varies from scale to scale." The reviewer made further calculations on the data presented and could find no evidence to support this contention. For example, on the morale scale, the third method excelled the first two in eighteen of twenty-eight comparisons. The comparisons were in terms of rank order correlations. Computations of sampling errors revealed that in not a single case were the differences significant. This does not disprove Sletto's hypothesis, but it does show that he has no evidence at present to substantiate the claim.

Astonishing variation was found by Sletto between groups for the weights assigned the items, regardless of the method by which they were secured. Thus, in constructing such scales, it is necessary that the population on which the test is standardized be a sample of the population to whom the finished scale will be applied. Description of the population on which the scale has been standardized should be included in the manual so that one using the scale may justify the application of it to his particular experimental group. This has long been known in mental test construction, but it is not so generally considered in the application of personality measures.

That a personality item may successfully discriminate on a number of scales is shown, and this is explained in terms of the scale value difference ratio. This ratio may be thought of as measuring the degree to which the factors determining response to a single item are identical with or equivalent to those determining responses to a given combination of items. This method, the scale value difference ratio, needs further study, particularly as to its efficiency in selecting homogeneous items as compared with correlation methods and the more elegant factor analysis technique. If it can be demonstrated that "pure" scales can be built by this method, then Sletto has made an important contribution. Otherwise, it seems to the reviewer, better methods of item analysis are available. This question is subject to experimental demonstration and should be examined in the near future.

Sletto devotes one entire chapter to the question of whether items yielding statistically significant differences measure the same factor. A consideration of the theory of analysis of variance would have answered this question in the negative. His careful epistemological discussion of the scale value difference ratio logically proves this; nevertheless, he feels he should and does bring experimental evidence to substantiate the point.

The type in this monograph is small, running five hundred words to the page. Furthermore, the type face is such that reading is not easy. Several typographical errors occur, one even of misplacing a complete line of type (page 54, paragraph 2). No attempt was made to check all the various computations, although the data for doing so is surprisingly complete. That they are not all correct was shown when the reviewer recomputed the rank correlations for the morale scale reported on page 36 and found small errors from $+.016$ to $.032$ in four of the six values. Nevertheless, the general impression was one of accuracy and dependability of conclusions.

Considering the study as a whole, the impression is highly favorable. The extent of the investigation, the thoroughness, and the critical insight into test problems evidenced in the development of the scale value difference ratio all support this impression. This technique, however, should be rigorously checked against other methods before adoption.

JACK W. DUNLAP.

University of Rochester.

WEISENBURG, THEODORE, ROE, ANNE, and MCBRIDE, KATHERINE E., *Adult Intelligence*. New York: The Commonwealth Fund, 1936. Pp. xiii+155.

During the last few years of Dr. Weisenburg's life he desired to apply certain mental tests to psychotic patients who had not been tested while they were healthy. He believed that it would be helpful to know what kinds of errors normal adult subjects made in the same tests. Hence, he caused these tests to be applied to some seventy hospital patients who were suffering from non-mental diseases. This monograph reports the result.

The first chapter is entitled, "A critical survey of studies relating to adult intelligence." It uncritically lists the reports, not distinguishing for example between formally defined intelligence, which enables students to learn, and a certain operationally defined test-intelligence, which was uncorrelated with their grades. In mentioning the test-intelligence scores made by individuals of different ages, as reported by Thurstone, Thurstone and Ackerson, Thorndike, Richardson and Stokes, Jones and Conrad, Willoughby, Miles and Miles, *et al.*, the authors commit the same fallacies of equivocation which I pointed out some years ago.¹

They then describe their tests, indicate the distribution of the scores in each, show the differences according to sex, age-level, educational level, etc., and give the intercorrelations among some of the tests. They append a list of 142 publications.

The work indicates that the authors are acquainted with certain writings and with certain technical operations employed by statisticians. This much might be expected of a master's thesis. I find in it little other information of value.

H. M. JOHNSON.

National Research Council, Washington.

¹ H. M. Johnson, Some Follies of 'Emancipated' Psychology. *Psychol. Rev.*, 1932, 39, 293-323. Cf. also, The Special Capacities of Adults for Continuous Education. *University of Virginia Rec. (Extension Series)*, 1935, Vol. 20, No. 4, 15-29.

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GESELL, A., and THOMPSON, H., *The Psychology of Early Growth: Including Norms of Infant Behavior and a Method of Genetic Analysis*. New York: The Macmillan Company, 1938. Pp. ix+290.

HARRINGTON, M., *A Biological Approach to the Problem of Abnormal Behavior*. Lancaster, Pa.: The Science Press Printing Company, 1938. Pp. 459.

JUNG, C. G., *Psychology and Religion*. New Haven: Yale University Press, 1938. Pp. 131.

MARSTON, W. M., *The Lie Detector Test*. New York: Richard R. Smith, 120 East 39th Street, 1938. Pp. 179.

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TRAXLER, A. E., *The Use of Tests and Rating Devices in the Appraisal of Personality*. New York: Educational Records Bureau, 1938. Educational Records Bulletin, No. 23. Pp. 80.

NOTES AND NEWS

DR. LOUIS WILLIAM STERN, professor of psychology at Duke University and former director of the Psychological Institute at Hamburg, Germany, died on March 27 at the age of sixty-six years.

AMONG honorary degrees conferred by Boston University on March 14, Founders' Day, was the doctorate of laws on Dr. Leonard Carmichael, president-elect of Tufts College. —*Science*.

THE following announcement has been received from President W. W. Atwood of Clark University:

"The department of psychology at Clark University has now been remanned by the appointment of two additional members. Dr. Raymond B. Cattell, who received his training at the University of London, has accepted the chair of associate professor of genetic psychology. Dr. Cattell received a large part of his advanced training under Professors Burt and Spearman. He has held a position for three years at University College, Exeter, England, and has spent four years as director of the City Child Guidance Clinic at Leicester, England. He was called to this country by Dr. Edward L. Thorndike of Columbia University and invited to devote his entire time for two years to research work in the fields of psychology in association with Dr. Thorndike. He is now engaged in that work, but he has made arrangements to accept the position at Clark beginning with the next academic year. Dr. Cattell has, for a young man, a long list of publications to his credit. In 1933 he published a book on *Psychology and Social Progress*, in 1934 one entitled *Your Mind and Mine*, a popular introduction to psychology for students. He has had another volume published entitled *A Guide to Mental Testing*, and he has also issued a set of group intelligence tests and a large number of journal articles.

"Mr. Donald E. Super, trained at Oxford University, England, and at Columbia University, New York City, has accepted the position of assistant professor of educational psychology. Mr. Super was born in Georgia, but has spent many years in Europe where his father is engaged in international Y.M.C.A. work. Since he was abroad during his high school and college days he attended the Coppet High School in Switzerland and then Oxford University, England,

where he received the A.B. degree in 1932 and the M.A. in 1936. He is now engaged in studies at Columbia which should lead to his Ph.D. degree. Mr. Super has already served as director of personnel and he is particularly interested in problems of vocational guidance. For two years he was on the faculty of Fenn College, Cleveland, Ohio, and in 1937 he served for a time on the staff of the University of Buffalo. He will return to that institution for the summer school of 1938.

"In association with Dr. Vernon Jones, Dr. Robert M. Brown, and certain members of the department of physiology these two men will carry forward the work in psychology and education at Clark University."

THE new *Journal of Neurophysiology*, edited by J. G. Dusser de Barenne, J. F. Fulton, and R. W. Gerard, made its first appearance with the January, 1938, issue. Its primary aim is to provide a channel for prompt publication of original work bearing on the functions of the nervous system, peripheral and central. Manuscripts should be addressed to the Editors of the *Journal of Neurophysiology*, 333 Cedar Street, New Haven, Connecticut. Manuscripts will be selected for publication in a particular issue of the *Journal* from those which reach the Editors up to the day of publication of the preceding issue. Final selection will be made within three weeks of this day and those articles for which there is no space available in the issue in question will be promptly returned to their authors. Only for special reasons will a manuscript be held for later publication if it cannot be included in the proper issue. The subscription rate for the volume of six bimonthly issues is \$6.00 in the United States, Canada, and Latin America, and \$6.50 in other countries; it is payable in advance to the publisher, Charles C. Thomas, 220 East Monroe Street, Springfield, Illinois.

COMPLETE sets of *Social Science Abstracts* for the four years from 1929 to 1932, inclusive, during which it was published, may be obtained from the Social Science Research Council upon payment of express and handling charges. These charges, to be paid at the time the request is made, amount to \$1.00 anywhere in the United States except California, Oregon, and Washington, where the amount will be \$1.50. For Canada, the charge will be \$3.00, and for other foreign countries, \$4.00. Communications should be addressed to the Social Science Research Council, 230 Park Avenue, New York, New York.

THE third meeting of the Conference on Methods in Philosophy and the Sciences will be held at the New School for Social Research in New York on Sunday, May 8. The morning session will be in the nature of a symposium on problems of method in psychology. The afternoon session will be devoted to brief papers on and discussion of the effects of the growing trend toward authoritarianism on the practice of scientific method and the application of its results. Anyone who is interested in attending this conference is invited to communicate with the secretary, Gail Kennedy, Amherst College, Amherst, Massachusetts.

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